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CONTRIBUTIONS TO THE ANALYSIS AND SYNTHESIS OF KNOWLEDGE

Published in cooperation with the
Institute for the Unity of Science

In accordance with the policy of the American Academy of Arts and Sciences to encourage publication of papers transcending the boundaries of separate disciplines, this issue of the Proceedings represents the first of a series of Contributions to the Analysis and Synthesis of Knowledge which the Academy expects to publish in cooperation with the Institute for the Unity of Science. This and subsequent numbers comprising the prospective series may be obtained independently of other issues of the Proceedings at the price designated on the inside of the cover by addressing the American Academy of Arts and Sciences, 28 Newbury Street, Boston 16, Massachusetts.

For the Committee on Publication

ERNEST H. HUNTRESS,
Acting Editor

INTRODUCTORY REMARKS

In his inaugural address on "The Future of the Academy", delivered in October, 1944, President Howard Mumford Jones pointed out that, while "all the forces leading to the fractation of knowledge have been more vigorously operative in the last forty years than ever before", there are two forces for unity.

First, "in some sense specialism has defeated itself. That is to say, what once looked like a separate compartment of knowledge, unique and apart from everything else, the more it is explored, the more its boundary lines seem to waver and vanish. The relation of fields is not that of pigeon-holes, but of living, interdependent cells. The historian depends upon the anthropologist, the anthropologist upon the sociologist, the sociologist upon the psychologist, the psychologist upon the biologist, the biologist upon the chemist, the chemist upon the physicist, the physicist upon the mathematician, and so on in an endless circle. . . . The very multiplication of specialties has forced the specialists to come together, to exchange ideas, to study each other's techniques, even—what is anathema to the old-fashioned, hard-boiled, realistic investigator—to discuss the philosophy of what they are doing."

"And a second pressure towards unity has come from the outside. Students of Western culture have freely said that if we do not recover a common set of ideas, a common tradition, and a common language, we are lost."

He then suggested that an Academy embracing all fields of knowledge, such as the American Academy of Arts and Sciences, was a particularly suitable instrument for intercommunication across the boundaries of the various disciplines, and asked whether it was not in fact the duty of this Academy to work toward the creation of "a language common to all disciplines".

Following these suggestions of President Jones, the Academy appointed a committee (under the chairmanship of Kirtley Mather) to develop concrete recommendations. The report of this Commission on the Present Status and Future of the Academy (October, 1946) indicated that "tacit in the smoothly rounded phrases of the Charter of Incorporation and other early documents is the thought that the spirit, purpose and essential logical and instrumental methodology of science can be applied more or less readily and successfully to any and every form and

aspect of knowledge." Moreover, the fact that the Academy "embraces all the scholarly disciplines . . . provides a basis for cross-fertilization among the various fields of intellectual endeavor and an impetus for synthesis of ideas and information, much needed to offset the extreme departmentalization of modern thought." The report said that "increasing specialization of knowledge is tending more and more to produce . . . 'uneducated experts' " and called for "humanization of knowledge" which does not mean an emphasis on humanities in contrast to the sciences, but an integration of the sciences with the humanities. "It transcends by far the idea of synthesis of knowledge as contrasted with the analysis of nature."

The "Institute for the Unity of Science" was founded in the year 1947. Its purpose was defined in its charter as "to encourage the integration of knowledge by scientific methods, to conduct research in the psychological and sociological backgrounds of science." The Institute represented a group of scientists and scholars (many of them members of the Academy) who felt a need similar to that so eloquently expressed by President Jones and by the report of the Mather Committee. The Institute is cooperating with the Academy in publishing a series of the Proceedings devoted to contributions to the "Synthesis and Analysis of Knowledge." By this title we wanted to describe in an abbreviated way two tasks which have been frequently regarded as contradicting each other. One has contrasted the logical analysis with the speculative synthesis of knowledge. According to our view, these two tasks complement one another and are fundamentally one and the same. By analyzing knowledge, we find the elementary operations of which our scientific knowledge consists. But we recognize that these elementary operations are essentially the same ones in all fields of science and learning. They can serve as the building stones for the synthesis and integration of our knowledge in all fields. This means the construction of a picture of the world which is speculative, in the sense that atomic physics is speculative. But it is analytical in the sense that the sciences of mechanics or genetics or economics are analytical. The field of research that we have described as "analysis and synthesis of knowledge" has been frequently called the "philosophy of science." As we work for an integration between the sciences and the humanities, we soon recognize the focal importance of the integration between science and philosophy. At this point, the departmentalization in our system of research and education has been carried to a self-

destructive stage: "philosophy", which in keeping with its tradition should take the leadership in the fight for integration, has become itself an isolated "department." We produce "doctors of philosophy" who are either ignorant in philosophy or ignorant in science.

One of the essential means of combating the prevalence of what President Jones called "uneducated experts" is to free philosophy from its isolated role and to restore it to its place as a link between the arts and sciences with the active cooperation of the workers in both areas. When August Comte in his "Positive Philosophy" attempted to establish a philosophy which should be essentially an instrument of integration, he claimed, not in vain, that he intended to restore to philosophy the place allotted to it by Aristotle.

This first issue of the new series of the PROCEEDINGS contains papers presented at the first national conference held under the sponsorship of the Institute for the Unity of Science at the American Academy of Arts and Sciences in April, 1950.

The plan of this meeting was to discuss some issues which have been focal points in the approaches towards an integration of knowledge. During recent decades, substantial progress has been achieved by considering the sciences as formal systems and by analyzing them from the logical and semantical viewpoint—this means from the viewpoint of their internal coherence and from the viewpoint of their meaning in the world of our experience. This kind of analysis is the topic of Parts II and III of the conference.

However, it has turned out more and more that these problems cannot be settled definitely on the basis of logical and semantical analysis. There remain always several possibilities for the choice of a formal system. Carnap contrasted recently in an excellent way the "internal" problems which can be solved by logic and semantics with the "external" problems. The latter ones put the question whether a certain formal system, as a whole, with the addition of a semantical interpretation, is useful for the orientation of man in the world of experience. Here we turn from the logical and semantical to the pragmatical viewpoint. Carnap stressed, for example, the point that the problem of whether one has to introduce "abstract objects" depends in the last analysis upon for what purpose one introduces them. The same resort to pragmatic criteria we find in Hempel's treatment of the problem of "meaning." In the last year, Quine has even advanced the opinion that also the distinction between

analytical and synthetical propositions depends ultimately on pragmatic criteria.

However, if we want to speak in terms of actual science, what kind of argument do we call "pragmatic"? To get the answer we have to consider science as a human enterprise by which man tries to adapt himself to the external world. Then a "pragmatic" criterion means, exactly speaking, the introduction of psychological and sociological considerations into every science, even into physics and chemistry.

It seems, therefore, that the sociology of science, the consideration of science as a human enterprise, has to be connected in a very tight way with every consideration which one may call logical or semantical. But, of course, we can only reach this cooperation by building up a sociological theory of science in addition to the logical one, and by working out both approaches until they will meet each other. For these reasons, the papers on "Science and Man" form the first part of this issue.

It is widely felt in the Institute for the Unity of Science that by the combination of the logical and the sociological approach to science all the needs which have produced traditional philosophy and metaphysics can be satisfied.

PHILIPP FRANK

GREETINGS TO THE NATIONAL CONFERENCE OF THE INSTITUTE FOR THE UNITY OF SCIENCE BOSTON, MASSACHUSETTS — APRIL, 1950

J. B. CONANT
President, Harvard University

PROFESSOR FRANK, LADIES AND GENTLEMEN:

I appreciate the honor of being asked to say a few words at the opening of this conference. For I feel sure that what you are endeavoring to accomplish in the formation of an Institute for the Unity of Science is of great importance. Both as a college president and a chemist I have personal reasons for being deeply interested in the unity of science. As Professor Frank has indicated in his kind introduction, I have recently experimented with the presentation of science to undergraduates as part of a general education. If we are going to give the lawyer, the statesman, the business man, the newswriter some understanding of science, we educators must ourselves endeavor to understand the methods of science.

As a university president I have had the privilege of sitting as chairman of *ad hoc* committees concerned with appointments to professorships in a great variety of fields of learning. Such an unusual educational experience enables me, however superficially, to know something of the accomplishments and ambitions of the scholars of today. The unity I see among all the learned men of our universities is not a unity of method, but a common agreement as to certain premises that almost unconsciously guide their actions. It is assumed that new knowledge is of the utmost importance, that a man's value to a community of scholars rests not on what he knows but on what new ideas he may contribute. I suggest that since the Renaissance, at least, it has been the dynamic nature of the undertaking that has provided the unity between natural scientists, grammarians, historians and philosophers.

I have spoken of new knowledge and new ideas. I take it one of the chief problems facing the whole learned world today is how to evaluate intellectual labor—one's own program as well as those of one's colleagues. Stamp collecting, as far as I know, has never been admitted into the academic circle. There are no chairs dealing with this subject, yet many men have labored long

and catalogued a vast amount of information about the symbols used in a highly important activity of modern man. Why not stamp collecting? I suggest this question might be considered by scientists and scholars from time to time. And lest this question seem to point a possible finger of scorn at those professors who turn over endless dusty pages of manuscripts or rare books let me address a question to my fellow chemists. Why not a professorship of the culinary art? Why has not cooking been added to the branches of applied chemistry taught in our universities?

Of course some of you will be quick to reply to these questions that some activities of scholars and scientists are little better than stamp collecting or cooking and should be purged from a true university. Perhaps your criteria will be in terms of definitions of natural science as a quest for the ultimate structure of the physical universe, of the humanities and social sciences as endeavors to understand the nature and destiny of man. I should draw the line between "proper" and "improper" academic subjects on a different basis, for my own definition of science as part of accumulative knowledge is not in terms of any final goal but the nature of the process. The essence of the undertaking of the historian, the social scientist, the philosopher, the natural scientist to my mind lies in the fact that his efforts will be measured by their fruitfulness, not by their finality. Even physical science, to my mind, is not a quest for certainty but rather a quest which is successful only to the degree that it is continuous. For science according to my view is best defined as "a series of interconnected concepts and conceptual schemes, schemes arising from experiment and observation and fruitful of new experiments and observations."

Science thus defined, I hardly need remind you, can be identified with the new "experimental philosophy" of the 17th century. The study of scholars of the last few decades has shown, however, that in the rapid growth of mechanics in the 17th century, the point of view of that century was not as new as historians once thought. Rather the type of reasoning that may be called deductive or geometrical was of prime importance and the history of this type of intellectual activity can be traced back well into the Middle Ages; it is, of course, well exemplified by the writings of Archimedes. The union of this type of scholarly activity with the cut-and-try type of experimentation long practiced by the practical men (craftsmen, agriculturists, artisans) produced modern science.

The union required many generations for completion and perhaps not before the late 18th century can we see the methods of experimental science in full swing. The method of "geometrical reasoning" is illustrated not only by the writings of Archimedes, but also by those of Stevin of Bruges and Pascal on hydrostatics. As to the practical arts, any number of examples might be chosen. The winning of metals from their ores is highly instructive. It is quite clear that the rules and recipes of the smelters of the 16th, 17th, and 18th centuries were highly satisfactory in terms of practical results. The men of those centuries were in command of a vast amount of organized (though hardly systematized) knowledge; they were able to identify rocks, "earths," in terms of what they would yield when smelted. All this information and the rules for procedure are quite similar to the recipes in a modern cookbook. Relatively few concepts are to be found in such books other than those we designate as "common-sense ideas" (e.g., spoons, the process of mixing, baking, etc., "salt," flour, etc.). For the metal makers of the 16th century the Aristotelian principles as modified by the alchemists provided some general points of reference ("sulfur," "earth," "fire," "water"). With the introduction of the phlogiston theory a first attempt was made at a working hypothesis on a grand scale which was relevant to the work of the "sooty empiric." From this broad hypothesis which was soon so widely adopted as to become a conceptual scheme, a whole host of deductions followed which could be connected with experimental findings.

The phlogiston theory reduced the degree of empiricism somewhat in practical chemistry (smelting) and was fruitful of much significant chemical work. However, practical men made relatively little use of the phlogiston theory; indeed, the practical books on metal making of the 18th century barely refer to any theory. With the advent of the new chemistry of Lavoisier and the atomic theory of Dalton, mineralogy and metallurgy begin to become "scientific." The degree of empiricism in metal making and in mineralogy dropped rapidly. But in spite of the developments of physics and chemistry from 1800 to 1950, metallurgy is still an art with a considerable degree of empiricism.

The distinction between scientific advance and empirical innovation is to be sought then in terms of what is usually called "theory." In reviewing the early phases of the history of each science the distinction is relatively clear-cut. But in this century so many concepts of science and so much of the conceptual scheme has become embedded in common sense that the distinction

is one of degree and not of kind. The definition of science as "attempts to lower the degree of empiricism in a given area" thus centers attention on those aspects of scientific inquiry that most clearly distinguish such activities from the empirical studies of the inventor. It likewise defines the boundary between science and such purely empirical endeavors as cooking and stamp collecting.

Of course, no investigator in his right mind prefers empiricism to theory. Everyone would like to have clear-cut operational definitions of all the variables, but when this is accomplished, the problem is nearly solved! Therefore, there is no use berating one group of scientists for their failure to use concepts that are clearly related to experiments or observations; it is only helpful if one can suggest specifically how the "fuzzy" nature of many scientific ideas can be made less "fuzzy." In the history of every branch of science the early investigators started with the current common-sense notions which were bound to be hazy criteria for identifying the variables and procedures. Often the best that even a genius can do is to try to relate one set of common-sense ideas to another set. He comes pretty close to arguing in a circle as did Pasteur in his study of fermentation. Very close to a circle, but not quite; rather a spiral argumentation, we may say! Vicious as this reasoning is from the point of view of logicians, it has proved in practice more than once to have been the mode of activity which opened up new fields. I venture to think that if Pasteur, for example, had been more deeply imbued with the "principles of right thinking," he would never have advanced biochemistry as he did. In other words, if one waited for clear-cut operational definitions and were unwilling to argue *almost* in a circle, progress in some areas might never be made at all. If this be true, it is of importance for the methods of science and significant for the application of the methods of science to fields now entirely empirical.

If one can judge the future by the past, the further reduction of empiricism in the biological sciences (including psychology) and the social sciences will come as men of genius have ideas of a general nature which can be related to observations and experiments, or as new instruments or techniques make available data which again men of genius can successfully use as a basis for new or enlarged concepts. In the meantime one must be content to use as intelligently as one can the empirical procedures which are available. This would seem to apply to organic chemistry, experimental biology, psychology, and the social sciences.

If this be true, exhortations to people to be "scientific" or to substitute "controlled inquiry" for judgments based on mere opinion would seem to be somewhat beside the mark. A farmer of intelligence engaged in buying or selling a horse is likely to be as free from prejudice or emotions as a scientist in his laboratory. A shrewd investor looking into the potentialities of a new business, a lawyer giving an opinion to his client as to the possibility of a court action, are all engaged in trying to form an impartial judgment. On the other hand, a scientist or engineer arguing for the construction of a pilot plant to demonstrate his invention may be carried away by that emotion often labeled as the pride of authorship. So too may be an educator who is arguing for his own pedagogic devices be they a definition of science or a set of words to relate science and invention.

With this last thought in mind, I realize it is time for me to conclude. I do so by once again congratulating the members of this conference. Whether you agree or not with my emphasis on the dynamic nature of science, I am sure you will agree that a quest for unity in science is a forward-moving enterprise. As one way of attacking the intellectual problems of this troubled century, your Institute is certain to be of service. All the members of the learned world must wish you well in this endeavor whether they agree with your basic philosophy or not. That your combined efforts may be fruitful of ideas yet unthought of is my own hope, as I bring you greetings here today.

PART 1

SCIENCE AND MAN

THE LOGICAL AND SOCIOLOGICAL ASPECTS OF SCIENCE

PHILIPP FRANK
Harvard University

1. PLATO'S VIEWS ON "SCIENCE AND GOVERNMENT"

In his book, *The Laws,* Plato discusses the attitude which an ideal government should take towards science. Briefly speaking, he regards science not so much as a search for truth as an instrument for the education of "good citizens". He deplores, like many educators and politicians of our own age, the bad influence of science on religious and political education.

In *The Laws,* the "Athenian Stranger" who advocates Plato's own views says:

> The theories of our modern men of enlightenment must be held to account for the mischief they cause. Now the effect of their compositions is this: when you and I produce our evidence of the existence of gods and allege this very point — the deity or divinity of sun and moon, planets and earth — the converts of these sages will reply that they are but earth and stones, incapable of minding human conduct, however plausibly we have coated them over with a varnish of sugared eloquence.

The Cretan, Clinias, replies to this, "A dreadful theory that you are talking of, sir, even if there were only one such! How much more dreadful our present age were such doctrines so abundant".

The doctrine which Plato regards as dreadful is simply commonplace today. In every elementary course in physics, chemistry or astronomy, we learn that the sun and planets are made of the same chemical elements we find on earth. However, Plato is concerned lest this dangerous doctrine of astrophysics lead to impiety, and the "Athenian Stranger" suggests measures to prohibit the teaching of such doctrines. He says:

> For the disobedient, a law against impiety may run as follows: if any man commits impiety of word or action, any person present shall defend the law by giving information to the magistrate. The first magistrate under whose notice the matter comes shall bring the

> case before the court appointed to deal with such offenses as the law directs.

In order to remove the danger that some official may be lenient towards such bad doctrines, the "Athenian Stranger" makes the following suggestion:

> Any official failing to take action on information received shall himself be liable to be proceeded against for impiety at the suit of anyone willing to vindicate the law. In the case of conviction, the court shall impose a particular penalty on the offender for each act of impiety. Imprisonment shall form part of the penalty in all cases.

We note here how much the attitude of the "ideal government" towards science differs from the attitude which a modern scientist would regard as "ideal". For the scientist, the first requirement is that the principles of science should be "true". This means, for the scientist, that these principles should be confirmed by sense-observations and logical conclusions. But the authorities who are responsible for the good conduct of citizens require, in addition to "scientific truth", that the principles of science should be useful, or at least not harmful, in the education of "good citizens". These authorities may be the state, the church, the school, or the opinion of the "man in the street". Their chief interest is not in the "truth" of a scientific doctrine but in its "goodness", as we may call it, meaning its usefulness for the education of "good citizens".

There is an inherent difference between these requirements for "truth" and for "goodness" in scientific doctrines. Those who make the first requirement judge a doctrine according to the "logico-empirical" criteria of science; those who make the second requirement judge a doctrine according to its "effect on the social behavior of citizens". One can, therefore, speak of judging a scientific doctrine from the "logico-empirical" *or* from the "sociological" aspect. If we mean by "logico-empirical" aspect the checking of a scientific doctrine by experiments and logical deductions, there is no doubt that Plato stressed the "sociological" aspect. We recognize this very clearly if we look into the seventh book of his *Republic* where he outlines a curriculum for the training of leaders in his ideal state. He speaks about the study of arithmetic, geometry and astronomy. One sees very clearly that he suggests teaching these subjects not as

systematizations of man's sense experience nor as methods to govern the physical world. He sees arithmetic, geometry and astronomy as paths by which the pupil can march towards the ideas of perfection, beauty and harmony. These ideas in turn will lead him towards the idea of the "good". And to be imbued with this idea is, according to Plato, the ultimate goal in the education of a "good citizen".

Plato proposes that the leaders in his ideal state must understand, for example, the nature of numbers not "like merchants or retail traders, with a view to buying or selling, but for the sake of their military use, and of the soul itself". The leader, as Plato conceives him, should be "soldier and philosopher in one". Speaking of geometry, Plato acknowledges its usefulness for the military art:

> An officer who had studied geometry would be a very different person from what he would be if he had not . . . But for such purposes a slight modicum of geometry and calculation would suffice. What we have to consider is whether the greater and more advanced part of it tends to facilitate the appreciation of the idea of the good.

Besides arithmetic and geometry, Plato recommends for the curriculum of the leaders or "guardians" of society, the science of astronomy. In this case also, he makes perfectly clear that the educational value of astronomy is not in its fitness as a theoretical basis for calculating the observed positions of the stars, but as a path towards the recognition of the divine nature of the sun and planets.

We see that Plato valued all sciences as instruments for moulding the mind of the student in such a way that his conduct would conform to the educational and political goals of the government. His emphasis was on "goodness", not on "truth".

2. Are "Truth" and "Goodness" Compatible Criteria?

If the authorities in government and education require that a scientific doctrine be useful for the education of "good citizens", it seems probable that they may object to doctrines which have been established by using only the criteria of logical correctness and agreement with observation. However, if a doctrine is confirmed "by science", meaning by observations and logical conclusions, how can its social usefulness have an influence on its acceptance? If a scientific theory is "true", we can derive from

it statements which can be checked by observation. These statements of fact are certainly determined by the theory. But, on the other hand, it is not possible to derive a scientific doctrine from observed facts. It is certainly possible that several scientific theories may account for the same observed facts. In this case, the "logico-empirical" criteria are compatible with two or more different theories. The authorities, state or church or public opinion, can select from among these doctrines the one which is the most useful for the training of "good citizens". To understand that the observed facts do not determine scientific doctrines unambiguously is the most important prerequisite for understanding the role played by sociological factors in the acceptance of scientific doctrines. Many people have claimed that there cannot be any influence of state or church on scientific doctrines because no authority can "change observed facts". This is certainly true. But an "authority" can require the abandonment or acceptance of scientific *theories* without requiring a "change of the facts".

The most conspicuous example of such a situation in history is the choice between the geocentric and the heliocentric world systems. When Copernicus advanced his heliocentric hypothesis, it was only a new way of deriving already known facts about the positions of the planets on the celestial sphere. The choice made by individuals was by no means dependent on their insight into astronomical research. It depended on their adherence to particular philosophical and theological creeds, or on their "common sense". Since no conclusion of astronomical science dictated an adhesion to one or the other world system, the authorities were able, without contradicting the results of "science", to enforce the choice of the geocentric system. Not only the Roman Church intervened in this way, but also, with the same energy, a number of the Protestant churches, particularly the Lutheran.

In this example, we see that the influence of "authorities" or, more generally, of social powers, of "existential factors" (Merton) on the acceptance of a scientific doctrine was made possible by a kind of "stalemate" between conflicting scientific doctrines.

A stalemate in the fight for "truth" usually results in the victory of the doctrine which is accepted in the competition for "goodness". But in this case, the firm establishment of Newtonian mechanics in the 18th and 19th centuries meant a clear victory for the "heliocentric, or Copernican, world system". Newton introduced the concept of "absolute space", and, according to the Newtonian mechanics, it was definitely certain

that the earth was moving relative to this absolute space. This defeat of the geocentric system, in spite of its "goodness", was only made possible by the decline of the ecclesiastical authorities and the rise of liberal governments. The Copernican system had become a symbol of liberation from church authority.

Since the end of the 19th century and the beginning of the 20th, relativistic mechanics and positivistic philosophy have developed, both connected with the physicist, psychologist and philosopher, Ernst Mach. He clearly demonstrated that to say that "the earth is moving" or "the earth is at rest" are merely two ways of describing the same facts. The choice of one or the other is a question of simplicity of description. Since the Russian Revolution in 1917, the ruling party of the Soviet government has regarded with strong misgivings any possibilities of justifying the decision of the Church against the Copernican system. They believed that the relativistic physics and positivistic philosophy could be used to influence the mind of the people in favor of the Church and other traditional authorities. The Soviet government suspected a harmful influence on the conduct of its citizens. This is one of the reasons why positivistic philosophy (often called "Machism") has been ostracized in the Soviet Union. The Soviet government has had to accept the "observed facts" in relativistic physics, but it has required a non-positivistic interpretation. On the other hand, the quasi-justification of the Roman Church by the new philosophies has led to some "flirtation" between positivism and the philosophies favored by the Church.

We note that whenever a decision is left open by science, this decision is resolved by churches, governments, or public opinion, in short, by "existential factors".

3. Mechanistic and Vitalistic World Pictures

There is a similar stalemate in biology between the doctrines of "mechanism" and "vitalism". Adherents to the doctrine of "mechanism" claim that physico-chemical laws alone can account for the behavior of living organisms, while adherents to the doctrine of "vitalism" deny this and believe in specific "organismic" laws. When I spoke once with Bertrand Russell about these alternatives, he said, "This is not a scientific but a political problem". As a matter of fact, we can see very frequently that political or religious authorities take sides in this issue because they believe that the "organismic" or the "mechanistic" hypothesis supports their particular philosophies.

E. Krieck, who had the authority to define the official philosophy of education in Hitler's "Third Reich", said bluntly: "The world picture fitting to the Third Reich is the organismic world picture". He connected the "mechanistic" world picture with the political doctrine of liberalism. He said: "In the field of science, the liberalistic illusion, which consists in teaching an arbitrary aggregate without sense and coherence, will be terminated". (E. Krieck, *National-Politische Erziehung*, 1933)

The Soviet government, on the other hand, has strongly objected to "vitalistic and organismic" theories as being encouraging to the medieval tendencies in church and society. The most radical form of "vitalism" is the belief in telepathy, the belief that between two human beings interaction can occur without any physical communication. J. B. Rhine, the most prominent advocate of telepathy in this country, has suggested that we might undermine the political philosophy of the Soviet government by propaganda on behalf of the vitalistic conceptions of biology and, particularly, on behalf of telepathy. He writes (in the magazine *Tomorrow*, 1950):

> If a human being is only a system of physical fields of force, if there is nothing at all in his make-up beyond the properties of matter and the energies of physics, we may as well admit right now that the Russian ideology will win in the end, no matter what our military strength may be . . . .

Rhine, like Plato, the Roman Church, the Nazis and the Soviet government, recommends a scientific theory because it is a good instrument in shaping the minds of people in favor of a particular political creed. He recommends vitalism and telepathy as weapons in the political struggle against Russia. He writes, "It would be downright insane to ignore anything that promises to split the foundation of Soviet philosophy wide open". He even argues that "mechanistic biology" has to be rejected by all advocates of "free enterprise". To regard human beings as "mechanisms" is incompatible with a belief in "freedom of choice". How right Bertrand Russell was to call the "mechanism-vitalism" alternative a political problem, not a scientific one.

Since the 18th century, Newtonian mechanics or the interpretation of the physical world in terms of this theory has been the backbone of the mechanistic world picture. Around 1900, however, scientists ceased to regard Newtonian mechanics as an all-

embracing theory of physical phenomena and recognized it as being valid only within a restricted domain. It is not applicable to high velocities or very small sizes. A tendency soon became noticeable to interpret this restriction of Newtonian mechanics as an opening wedge for an organismic and vitalistic biology and even for an organismic physics. The merely statistical character of the new laws was interpreted as indicating the existence of "freedom" in the vitalistic and organismic sense. A great many authors have believed that the new theory confirmed the kind of freedom and responsibility of the individual which traditional jurisprudence regards as a necessary basis for a just punishment of criminals.

Many educators, clergymen and judges have hailed this interpretation of science as a valuable support in the education of a "good citizen". As a necessary consequence, some pressure has been exerted by those authorities to encourage this interpretation.

Those who are not very critical take it for granted that modern science has "proved" the freedom of the will. On the other hand, there are some educators (I include under this category also religious and political leaders) who know approximately what it means to say that a scientific doctrine is "proved". It means that the general statements of this doctrine are compatible with the observed facts. In our case, the opposite statement seems also to be compatible. In a certain sense, one would say that from the scientific viewpoint the freedom of the will could either be accepted or rejected. We have here again the scientific "stalemate" that gives rise to the influence of what one may call "moral pressure groups". A very instructive example of this is given by the prominent philosopher of law and liberal judge, Jerome Frank. In his book, *Fate and Freedom*, he takes for granted that science cannot decide whether there is "freedom of will" or not. Now the educator comes in. Since, according to his opinion, belief in "freedom of will" supports the "American Way of Life", we have to make use of the leeway provided by science and accept "freedom of will" as a statement within our system of accepted truths. I could add a great many examples of a similar type.

There is a widespread opinion that the influence of religion and politics on science occurs only in strictly "authoritarian" countries. The Roman Church, the Hitler government in Germany, the Soviet government in Russia have been accused of making political considerations reasons for the acceptance of a

scientific theory. My examples show clearly that the intention to use scientific theories as material for the education of good citizens is as active today as it has ever been. This intention will exploit every stalemate between conflicting scientific doctrines.

4. "Real Things" and "Abstract Concepts"

A distinction has often been made in the general principles of science between concepts which denote "real objects" and other concepts to which no "real objects" correspond. The latter are referred to as "abstract concepts" or "auxiliary concepts" or "constructs" which "exist only in the mind" of the scientist. For example, there has been in philosophy a centuries-old dispute between "atomism" (or mechanism) and "dynamism". The first doctrine maintained that "matter" (divided into atoms) is a real thing while "force" is only an abstract concept. "Dynamism" asserted just the opposite. In the 19th century, the concept of "force" was replaced by "energy" and "dynamism" by "energetics", but the mediaeval dispute was continued under the new names. Similarly, in the theory of electromagnetism, there has been the dispute as to whether the "electric charge" is "real" and the "field" only an abstract concept or vice versa. In the 20th century, there has been the controversy whether the *psi* (Schrődinger) function is a "real object" or only an abstract concept.

A similar question has played a role in the foundations of Relativity Theory. This is whether "space" and "time" are real things or only auxiliary concepts which the scientist uses to bring order into his sense impressions.

In the social sciences, there has been an old dispute about whether the individual is a "real thing" while the "nation" is merely an abstract concept, or whether the "nation" is a "real thing" while the "individual" is merely an abstract concept. In this case, we see clearly that by saying that a certain concept denotes a "real object", we give it more importance than by calling it an "abstract" or "auxiliary" concept.

Professor C. J. Ducasse has pointed out frequently that the predicate "real" always means a predilection for the use of some concepts. We can easily find very drastic examples of the use of the terms "real" and "abstract" in the service of political doctrines.

Danislewski, a leading theoretician of the 19th century Slavophile movement in Russia, wrote that "mankind" is a product

of "abstract generalization" while "nation" is a "culturo-historical type" and a product of "intuition and personal experience". Such statements have the clear purpose to degrade love for mankind and to extol nationalism. But they claim to be based on the results of social studies, anthropology and sociology.

We learn to understand the role of such general statements in the social sciences even better if we look at a statement written by Joseph Goebbels in 1927, six years before he became minister of propaganda and culture in the Hitler Government. Goebbels wrote:

> The nation is a part of mankind. Mankind is no thing in itself and the individual is no thing in itself, either. *The thing in itself is the nation* . . . The materialist regards the nation only as a means to an end and will not recognize it as a substance by itself. For him the nation is an intermediate step between man and mankind; mankind is for him the end . . . Therefore the materialist is necessarily a democrat. The idealist regards the concept 'mankind' only as an abstract concept. Mankind is something imagined only, not a fact.

In the last sentence, the predicate of "being a fact" can certainly not be confirmed by any comparison with observations. It actually means only that the author of the statement does not like to work for mankind and does not want others to participate in international work.

In a similar way, the predicate "thing in itself" is ascribed to a subject as if it were a property which could be confirmed by some kind of experience. Actually, it expresses only the author's predilection for the subject.

5. Are There Alternatives in Modern Science?

Many scientists like to point out that it does not actually happen that the observed facts can be equally well explained by different theories. They say that in any period there is one theory which is "accepted by the scientists". This is certainly not the case if we understand by "theories" a system of statements on the highest level of abstraction. Even in the field of mathematical physics and astronomy, we don't know whether the assumption of a finite universe or of an infinite universe checks better with observed facts. The same situation prevails in most questions of cosmology. All phenomena which are explained

by the Theory of Relativity can be just as well explained by the existence of an ether. In the latter case, all changes in clocks, for example, are physical consequences of their motions relative to the ether. As for Quantum Theory, according to groups of scientists around Einstein and Schrödinger, it is not "proved" that the statistical laws of Quantum Theory are the most general principles from which the quantum phenomena can be derived. Perhaps these laws are just rules about the average behavior of particles. The final laws may be field equations which we have to average in order to obtain the usual Quantum Laws.

The situation is even more obvious in biology. We don't know whether the observed phenomena can be derived from the assumption of spontaneous generation or from the principle that life can only originate from life. If there is spontaneous generation, living organisms may originate from inorganic matter by pure chance. The question arises whether this is plausible or whether biology has to invoke some "plan" or "purposiveness" in the world by which changes can take place which are not likely to happen by "chance". This alternative "chance" or "purpose" cannot be decided by reference to observed facts or by "scientific research" in the ordinary sense of this word.

All the political doctrines which advocate a return to the political and social conditions of the Middle Ages show a definite predilection for one of these alternatives. They advocate "purposiveness" and reject "chance" as well as "mechanistic determinism". We mentioned in Section 3 that E. Krieck, the official spokesman of the "Third Reich" in education and philosophy, declared the "organismic world picture" to be the German world picture. According to him, "in the German universities only such men are entitled to teach and to do research who are committed with their whole personalities to the German world picture". (1933)

Oswald Mosley, the leader of the British fascists, writes in his book *The Alternative* (1947):

> The more intricate the pattern of the Universe, as demonstrated by modern physics, the harder it becomes to believe that the whole vast mechanism assembled itself by chance . . . We believe that it is now possible to derive from the actual evidence . . . some idea of the pattern of God . . . It is certainly clear that the purpose of this earth is a progressive movement from lower to higher forms.

Thus the choice between alternatives left open by science has been exploited for the justification of racial discrimination which helps the universe in its "progressive movement to higher forms".

If we consider the question whether the hypothesis of an "ether" should be abandoned in physics or not, we note easily that political influences have been active in bringing about a decision. In the first years of the Soviet government, the representatives of official Soviet philosophy denounced the Theory of Relativity as "idealistic" and "reactionary" because by dropping the "ether" the principle of "materialism" was dropped; that all observable phenomena have a material basis. On the other hand, the official spokesmen of the Hitler government in Germany denounced the Theory of Relativity as "materialistic". They claimed that the "ether" was not a strictly material substance and that Einstein had dropped the "ether" because he was a confirmed materialist to the degree that he was not willing to accept an "immaterial substance" like the "ether" as a basis of physical phenomena.

In the other cases of stalemate in scientific argument, political and religious pressures have also been brought to bear in favor of one or the other of the alternatives. Soviet philosophy has denounced the cosmological theory of a finite space, propaganda of organized churches has denounced spontaneous generation, advocates of "freedom" have denounced the strictly causal interpretation of Quantum Theory in order to clear a path for "freedom of will".

All this exploitation of stalemates in science makes a fascinating topic of research in the sociology of science which has been explored very little as yet.

6. "Passive" and "Active" Positivism

We have learned that every stalemate reached in the application of the usual scientific method provides an opportunity of a choice between alternative hypotheses. This choice is "free" in that it is not determined by the advance of science in the ordinary sense. The existence of the "ether", for example, cannot be proved or disproved by the methods of physical science as practised by the professional scientist. We have also learned that this "freedom" of choice can be exploited in favor of social, religious or political creeds. For this reason, all groups who are interested in mustering "scientific" support for their creeds try to resolve such stalemates in science. The scientists are urged to admit their failure and to beg for help from philosophers or

theologians. The declarations of the Holy Office in Rome, concerning the Copernican conflict, told the scientists that they could not do anything except collect observational material. But by this work, the choice between the two conflicting world systems could not be decided. Therefore, the scientists had to leave this decision to the philosophers and theologians. J. Maritain, who did a good job of making medieval philosophy palatable to the modern scientist, wrote bluntly that philosophy is dependent upon science only in the sense in which a lady is dependent on her chambermaid. In other words, philosophy takes only its raw materials from science but reaches its decisions on other grounds. Thus the Church took a view we would call today a "positivistic view". Scientists were warned to keep to the strictly logico-empirical method and to refrain from all audacious "speculation" which was to be reserved for "higher" types of knowledge. It is well known that the Church took the same stand, perhaps even in a more drastic way, with regard to the Darwinian theory of evolution. The advocates of the Church have recommended an extremely sceptical attitude to biologists and have advised them to draw a sharp line between observed facts and speculative thinking.

I must admit myself that I have learned a great deal from the anti-Galilean and anti-Darwinian writers who have tried to bolster up the cause of the Church. They have stimulated critical thinking and a clear-cut separation of speculation from facts. However, their aim has been to "clear the deck" for sustaining "superscientific" argument or "metaphysical argument" as it is called.

We notice a very similar attitude toward the sciences when we direct our attention to the National Socialist Government in Germany. The Government and the ruling party were concerned by the fact that generalizations had been developed in modern physics (in particular, Relativity Theory and Atomic Physics) which did not seem to match well with the political philosophy of the Third Reich. Therefore, the Government convoked in Seefeld (Tirol), November 1942, a conference in which leading physicists and political experts should develop a kind of compromise. The result was very similar to the compromise between the Roman Church and the adherents of Copernicus. Science had to be "contained"; it was authorized to "collect" facts but not to interpret them (in a very general way). A resolution was passed in Seefeld according to which modern atomic physics was authorized as a description of observed facts under the condition that

the scientists refrain from making any statements about the "constitution of matter". The underlying assumption was, of course, that authentic knowledge about the "constitution of matter" would be forthcoming from some "superscientific" source.

We can see that generally the powers that have been interested in controlling human conduct have envisaged an attitude of "humble positivism" among scientists. Scientists should simply collect material and leave the undecided questions of science to be resolved by the educators and rulers of men in such a way that the result will support an education for a "good life". But some scientists do not believe that "superscientific" methods of knowledge can answer the questions concerning which science has reached a stalemate. These scientists believe that where science has no answer, no one else has any answer, either. One has to wait until a scientific solution is reached. The decisions encouraged by social powers do not offer us any "truth". They are instruments of propaganda for a certain way of action. Whether one approves such an interpretation of science depends only upon whether one approves the way of life which is to be supported. This attitude is not "humble positivism" or "passive positivism" but we may call it "proud positivism" or "active positivism". This school of thought refuses to recognize the solutions reached by "superscientific" methods. It examines the decisions favored by social and educational influences and attempts to interpret them as acts which can be derived from the laws of human behavior.

7. Can Science Be Its Own Philosophy?

All the educational groups (in the widest sense of this word) who have encouraged and even cajoled "humble positivism" have rejected and flayed every attempt at "active positivism". The latter would frustrate all attempts to provide political and religious propaganda with the support of science. Jacques Maritain in his book *The Degrees of Knowledge* praises "humble positivism" because it teaches science to "know its place". But he attacks those scientists who have attempted to make of positivism, as Maritain puts it, a "philosophy of nature", or in the sense of August Comte, a "Positive Philosophy".

All of these groups have attempted to limit science to the collection of material and to enforce a "philosophical interpretation" which supports the "way of life" which they regard as a "good" one. For this reason, they will insist upon an autonomous philos-

ophy that will provide decisions where science, in the narrower sense, has reached a stalemate. Organizations like the Roman Church or the Third Reich in Germany have denounced in the strongest possible terms the claims of "active positivism" or, as Maritain puts it, of "scientism" to discuss the high-level generalizations of science in scientific terms.

We find the same attitude toward "active positivism" in the announcements of the Communist Party in the Soviet Union. They require positively a philosophy which is to control scientific work and which can be controlled itself by "existential factors". This means, practically, control by government, church or public opinion. On the other hand, the autonomy of science is denounced. If we read the articles "Philosophy" and "Natural Science" in the official *Little Soviet Encyclopedia,* we find the following formulations of Russian Positivists emphatically denounced: "Philosophy overboard" (Minin); "Science is its own philosophy" (Boritchewski); and "Philosophy consists of the most general results of the natural sciences" (Stiepanov).

Thomistic philosophy which has become more and more the official philosophy of the Roman Church points out bluntly that there is a superscientific knowledge which is provided by philosophy. Msgr. Fulton J. Sheen says in his book *Philosophy of Science* (1934): "No science proves its first principles, but accepts them from a superior science, until finally one reaches metaphysics, which defends its own principles". One of the principal advocates of Thomism in the United States, Mortimer Adler, stresses the point that philosophy is independent of the changes in science. He writes in his little book *St. Thomas and the Gentiles:* "Philosophy is a body of knowledge, not attained by any of the specialized methods of empirical research, and consisting of truths which are not dependent for their validity upon the findings of science." Since obviously philosophy cannot advocate principles which contradict the observed facts which are found by science, Adler's statement can only mean that philosophy, as an autonomous body of knowledge, can reach a decision when and where a stalemate in science is reached.

There are many scientists who are willing to follow the advice to accept a "humble positivism" as their own doctrine and to leave higher generalizations to "educators" who reach their decisions by considering "philosophical implications". But there is a difficulty which is very often ignored by those who accept this view.

A. N. Whitehead in the introduction to his book on the Theory

of Relativity points out that a scientist who tries to eliminate every kind of "philosophy" from his presentation of science will necessarily be influenced by the chance philosophy that he has imbibed as a child from his nurse or his Sunday School teacher or perhaps from the structure of his everyday language. Also, the average scientist frequently ignores this fact, but there have been quite a few prominent scientists who have emphasized that every presentation of science on a high level of abstraction is imbued with a certain philosophical interpretation which is not a result of scientific research. It may be a mechanistic or organismic or materialistic or an idealistic interpretation. The real meaning of these interpretations in physics or biology cannot be found in the sciences of physics and biology. But this fact does not imply that these generalizations reflect some superscientific knowledge. The real meaning of these generalizations can be found within science if we include also the social sciences in the system of the sciences. The idea of a superscience called philosophy or metaphysics arises only if we mean by "science" some special science instead of the system of all the sciences. The fact that no special science can, as F. J. Sheen said, "defend its own principles" does not lead to the conclusion that the system of *all* sciences cannot do so. The refusal to draw such a conclusion was the original idea of the "Positive Philosophy" as it was outlined by A. Comte and his disciple, J. Littré. They claimed that cooperation between the natural and social sciences would reach the objectives that were formerly reserved to philosophy. Their idea has been advocated more recently by men like J. Piaget and J. Dewey. John Dewey in his book *Reconstruction in Philosophy* (1920) presents the case for "active positivism" in a strong and impressive way:

> The history of philosophy will take on a new significance. What is lost from the standpoint of would-be science is regained from the standpoint of humanity. Instead of the disputes of rivals about the nature of reality, we have the scene of human clash of social purpose and aspirations.

INTEGRATION IN THE BIOLOGICAL AND SOCIAL SCIENCES

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As I pondered recently on what to say to a then imagined assemblage of intelligent integrators on the subject of integration in the biological and social sciences, it seemed somewhat of a pity that any need for such a discussion should appear to exist. For surely the essential oneness of biological and social science is sufficiently obvious. It is obvious, that is, if the interrelationships among human beings do in fact offer a proper field for realistic inquiry, or for that kind of unprejudiced inquiry which men have recently learned to address to the behavior and to the bodily functioning of their fellow beings, the other animals.

Most of us here today would readily grant that, to begin with at least, the proper study of man is biological man. For most of us the concept of a functional continuum extending from organic structure to observed behavior seems as natural when applied to the behavior of human beings as to that of other living organisms. Yet we are living in a society founded on basic tenets diametrically opposed to such an outlook, and the psychology underlying our institutions is not in fact biologically oriented at all. The rationalizations of life on which our current institutions rest are fundamentally mystical and supernatural, so perhaps it is not greatly to be wondered at that the species appears to have lost its way, in a biological sense, and to have drifted into an age of both hebephrenic and Dionysian confusion. The mixture of a basic philosophy of insolent supernaturalism with suddenly developing destructive power is a little like that other recently discovered bad mixture of alcohol with gasoline. The disastrous consequences are being felt in economic, sociopolitical and reproductive affairs as well as in religious and educational affairs, and we seem to be steadily approaching a major crisis. The crisis may be followed by smoother seas for a long season. It may be followed by social psychosis and chaos.

I think it has been growing increasingly plain that the situation calls for a biologically oriented psychology, or one taking for its operational frame of reference (its starting point) a physical anthropology expressed in terms of components (variables)

which can be measured and quantified within the same terminological system at both the structural and behavioral ends of the structure-behavior continuum.

The general philosophy and plan of social control — the *humanics* resulting from acceptance of that sort of psychological outlook—might be called a *biological* humanics. It seems to me that the crux of your problem of integrating biological with social science is simply the lesser but more acute problem of bringing yourselves to accept fully, in your social philosophy, the implications of a biological humanics. I suspect that not a tenth of one percent of American biological scientists today, let alone the social scientists, have really faced these implications and have accepted the personal responsibilities they entail.

Perhaps the most direct way of putting the matter would be contained in two propositions: 1) that integration between biological and social science necessarily implies a continuity between methods used for studying structure and those used for studying behavior. 2) That our whole present structure of social science, ignoring as it almost always does the factor of describing taxonomically the structural variation of individuals, is like a house built on sand. In practice social scientists do not, and in particular sociopolitical leaders who are wielding power and influence do not, show much awareness of the implications of a biological humanics.

It may be that so far as the colleges and the sophisticate literature are concerned the battle for verbal acceptance of the idea of the biological evolution of man has been won. But the implications of the idea have scarcely as yet reached the general consciousness at all. We should not forget that only a generation ago there was still a somewhat vigorously expressed difference of opinion, notably in Dayton, Tennessee, regarding the historical relationship of man to his earth and to his fellow animals. Certainly the implications of a biological humanics were as foreign to the minds of the Bryan cohorts at that Dayton trial in 1925 as vegetarianism is to the mind of a healthy tomcat. For those people an integration of biological with social science would have been tantamount to an integration, in the mind of the tomcat, between a taste for young robins and a taste for stewed burdock leaves.

A true integration between biological and social science would seem to call for one of three alternatives: Either acceptance by social scientists of the meaning and consequences of a biological humanics; or a similar acceptance among biologists of the con-

sequences of the theological humanics now partially prevailing in current ratiocinations of the social institutions and of current conventional social attitudes; or elaboration of some kind of formula of compromise. I should say that the philosophical history of the present century has been essentially a chronicle of abortive effort in the direction of the third alternative — attempted compromise. Space does not permit here any attempt on my part to summarize that story. I think it is a dismal story, in fact a shameful one, and I suspect it is possible that more harm has been done to the human cause during the past half dozen decades of abortive compromise (and consequent essential dishonesty) than during the preceding millennium and a half of predominantly honest theological insolence.

Our present institutions and the social attitudes contributive to them do not in fact rest on an honest theological humanics but on a vague compromise which represents ground almost certain to be abandoned as soon as a political focus (world order) is achieved. The philosophical choice will then be narrowed to the first two of the alternatives just now listed — either wholehearted acceptance of a biological humanics or a wave of hebephrenic jettisoning of human responsibility and another frantic scurrying back to some supernatural theology. This latter eventuality may happen before we know it. As a species we may have been growing biologically degenerate at a rate faster than anyone can believe, and we may suddenly scuttle to cover like a swarm of rats caught in a twinge of agoraphobia. It takes good men to face the implications of a biological humanics.

I suppose the term social science implies some degree of acceptance of responsibility for social control, not mere description or mere prediction. Certainly biological science carries that implication with respect to its own subject matter. Scientists are conative animals as well as cognitive and affective animals, and even for a scientist the purpose of interfering with a series of events is partially that of amplifying power over further events. Biologists however are usually in position to play down the conative side of their motivation, and to escape any consequences of it by proclaiming a remote disinterestedness. Social scientists ordinarily cannot do that and thereby they face a far more difficult situation. That is, they face it when they are both intelligent and courageous.

Social science is by its nature almost immediately *participative* in human affairs and controversies. Therefore social scientists

have to face an almost immediate accounting for their decisions, their commitments, their expressed ideas. If therefore a social scientist offends the conventional mood in any way, and gets any attention, he is almost automatically under devastative attack. A biologist is less likely to be understood, and so has a better chance to survive a radical departure from the conventional bad habits of his time. This is an advantage the biologist derives from working with material one or two steps removed from the most immediate emotional concern of members of his own species. The biologist is in somewhat stronger position than the social scientist and can indulge himself in a greater degree of independence of thought and attitude. This may be why biologists have been largely resistant to philosophies patently absurd, such as those postulating wishful shortcuts to human salvation, even when the latter have been hysterically popular. In any case it looks as if the impulse for integrating biological good sense with the practical problems of social control will be more likely to find favorable soil among biologists than among the social science people.

All this is only common sense. What is more important, and far more often forgotten in our present culture, is that the "impulse for integrating" is itself a product of neither biological nor social science, but is religious in nature. We are facing an essentially religious difficulty and if we are going to solve it we are going to have to treat profound religious pathology. If the job is to establish the continuity between biological and social science major attention needs to be given to the pathology which tends to break that continuity. Now my diagnosis of the pathology is this: The continuity between biological and social science appears to be broken wherever the oneirophrenic proposition is entertained that bodily structure could be one kind of thing while mind or temperament or the unconscious or the genetic pattern could be something else independent of it or out of the continuum with it. In short, it is the mind-body idea, or the psychosomatic idea, that appears to lie at the bottom of religious pathology.

I am not concerned with denying the possibility of the truth or ultimate rightness of any dualistic hypothesis. Indeed I suppose that ultimate truth may well reside in that quarter despite my hope to the contrary. What I do deny is the pragmatic usefulness of such an outlook. It seems to me a schizophrenic and a biologically unhealthful business, and it leads to postulation in the human behavioral hierarchy of causal entities that are out-

side the natural law. These "wild cards" destroy the integrity of the game, and they greatly hinder the development of a natural history of mankind. It is as a naturalist, not as a philosopher, that I protest against loading the deck with wild cards, for I love the order and system of nature and can see no reason for departing from it in our consideration of the human species.

At the Constitution Laboratory we make the philosophical assumption that human behavior, whether physiological, hormonal or mental, is always structure in action. In so doing I suppose we follow Vaihinger's philosophy of *Als Ob,* although reversing the premise. You will recall that Vaihinger suggested we proceed *as if* Mr. G were running the show and were carrying the responsibility for it. As a way of life that proposition offers many ameliorations. Particularly, it turns the naturalists and the biologists out of the temple and directs their attention away from the dignity and nobility of the human estate, for human ethical affairs are then exempt from the curiosity of naturalists and are Mr. G's business. More especially, the human body can then be somewhat disregarded and the quality of mind or soul, stemming as it does directly from Mr. G, is most emphatically *not* a reflection of the structure of the body or of the quality of the gut. Indeed, the continuity between biological and social science is broken altogether.

Vaihinger was perfectly realistic about ultimate truth. Admittedly he had a somewhat irreverent or possibly unchastened way of referring to Mr. G as God, thus calling the deity too familiarly by his first name, but he had no illusion of ultimate or immediate knowledge of Mr. G. He merely liked the idea of avoiding the task of building a biological or naturalistic foundation for social science, and he preferred to live *as if* Mr. G had already worked out the problems of social science.

That in fact has been the prevailing and I think insolently unhumble social philosophy of our comparatively recent past. At the Constitution Laboratory we are at least more humble, and we call Mr. G Mister. Everybody else is Doctor, which is German for daughter (when gutturally pronounced), is French for teacher, and has numerous other well-distributed meanings in Webster's unabridged dictionary. These include 1) a learned man, 2) a philosopher, 3) a physician, 4) a wizard in a savage tribe, 5) a prong-tailed skate, which is a fish, 6) a donkey engine, 7) a device for polishing a seat (presumably a swivel chair seat), 8) a pump in a river steamer, 9) a cock, 10) a loaded die for shooting craps, 11) a fixer of anything, 12) an artificial fly. The

term Doctor can thus be safely applied to all women and to nearly all men, while Mister is reserved for the most extraordinary aristocracy.

Meanwhile it will be remembered that Occam, early in the fourteenth century, adopted a view directly opposed to that of Vaihinger. Like the latter he laid claim to no mystical knowledge of ultimates, but in view of this admitted ignorance he felt that the way of choice was to live and think *as if* no mystical ultimates were likely to interfere with our human show, or were likely to be interested in it. *"Entia non sunt multiplicanda praeter necessitatem,"* he said, and this cryptic suggestion, which is Occam's statement of the Law of Parsimony, has been called Occam's razor. Entities (hypotheses) are not to be multiplied beyond necessity.

I suppose that one of the most urgent needs confronting the human species is the need for recording the *overt structure* of living individuals in such a way as to make possible a more systematic and more fruitful study of structure-in-action, which is actually the subject matter of chemistry, physiology and medicine and must become (if we are to attain the continuity we seek) the subject matter also of psychology and the social sciences. Perhaps if we elect to proceed philosophically *as if* it were known, a thing given, that organic structure in action is personality (also temperament, mind, and in fact behavior whether conscious or unconscious), if we can resolve as philosophers to operate on the basis of that drastic *Als Ob* assumption, perhaps we can even yet repair the broken continuity between biological and social science. My dark presentiment is that only if we can do it will the great schismatic tragedy of this age be overcome and final human disaster for the time being averted.

THE SCIENCE OF MAN AND UNIFIED SCIENCE

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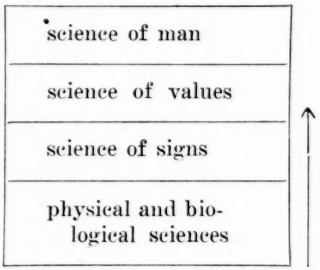
In 1939, at the Fifth International Congress for the Unity of Science, held at Harvard University, I read a paper with the title "Semiotic, the Socio-Humanistic Sciences, and the Unity of Science."[1] This paper suggested that "the theory of signs (semiotic) furnishes the key to the incorporation of the socio-humanistic sciences in the structure of unified science." The proposal made was to develop the theory of signs upon a biological base, to use this theory in the construction of a biologically based theory of value, and then to approach the whole domain of man's cultural activities in terms of semiotic and axiology so constructed. In this way it might be possible to build a bridge between the natural sciences and the science of man, both of which would then fall within a single system of knowledge. Such a program may be represented in the following diagram:



In the last decade much scientific work has been done which is relevant to the carrying out of this program. It is these developments which I wish to comment upon at this time.

THE SCIENCE OF SIGNS

The speaker's book, *Signs, Language, and Behavior,* published in 1946, was an attempt to show that semiotic could be given a biological foundation such that scientific methods could be applied to the whole domain of meaning. The suggestion was made that the analysis of any sign might be given by stating the things to which it directs behavior and the behavior which it prepares to these things. The interpretant of a sign (the effect of the sign

on an organism) would then be a disposition to react to something or other in a certain way, and the signification of the sign would be given by specifying the reaction thus prepared and the things to which the reaction is prepared. On this approach the basic terminology of semiotic would be constructed upon terms of the physical and biological sciences, and in principle the whole domain of meaning would be open to study by scientific methods.[2]

In this book it was further argued that such a behavioral approach to semiotic would allow a proper place for formal logic and logical analysis, since the approach itself furnished the basis for a distinction between analytical and non-analytical sign combinations. Symbolic logic could then be conceived as the analytical portion of semiotic. And since logical analysis is not a theory of meaning but an analysis of given or entertained meanings, it does not itself decide between various theories of meaning; hence it is compatible with a biologically grounded semiotic. Further evidence for this compatibility may be found in the work of Charles Peirce (especially volumes II and V of his *Collected Papers*) since Peirce too ultimately viewed the interpretant of a sign as a disposition to respond; in Book II of C. I. Lewis' *An Analysis of Knowledge and Valuation*, which grounds the ultimate distinctions needed by symbolic logic upon this Peircean view of meaning; and in Hans Reichenbach's *Experience and Prediction*, which finds no conflict between the pragmatist's theory of meaning and the analyses made by the logical empiricists. Here at a critical point in theory the approaches of the pragmatic and logistic wings of the unity of science movement seem to have converged through the development of a study of meaning.

Among the many steps toward a scientific semiotic which are taking place today, three may here be noticed. The basing of linguistics upon the theory of signs, as in the work of Roman Jakobson (see his *Sound and Meaning*), locates linguistics within semiotic, and this, taken together with the similar movement in respect to formal logic, augurs closer cooperation between the linguist, the logician, and the semiotician. Hans Reichenbach's attempt in *Elements of Symbolic Logic* to apply symbolic logic to the analysis of the everyday language is an instance of the direction of such cooperation.

The application of quantitative methods to content analysis by Harold D. Lasswell and his co-workers, while at present restricted to political discourse, is of sufficient generality to provide a model for the quantitative analysis of all types of discourse

(see *Language of Politics,* by Harold D. Lasswell, Nathan Leites, and associates). Content analysis, so conceived, falls within the empirical or descriptive part of semiotic, and makes more precise its methods of study.

The elaboration of the theory of communication by mathematical, physical, and engineering techniques (as in Norbert Wiener's *Cybernetics;* Claude Shannon and Warren Weaver's *Mathematical Theory of Communication*) is further evidence that semiotic has now been linked to the basic scientific and technological disciplines. It is possible that the concept of feedback mechanisms stressed by Wiener and his associates may provide, in the form of circular neural processes, an approach to a fundamental problem of semiotic: the nature of the interpretant of a sign.[3]

The Science of Value

A central stimulus toward building a science of value on a behavioral basis has been the work of John Dewey. In his *Theory of Valuation*[4] he envisaged the task of the theory of value as the study of the nature and relation of prizing and appraisal. In his article "The Field of 'Value' ", in *Value: A Cooperative Inquiry* (edited by Ray Lepley) Dewey uses the term "selective-rejective behavior" to characterize the general field of behavior within which the study of values is to be carried on; and in this article he maintains that an appraisal (or evaluation) does not differ in kind from a statement, and hence is controllable by scientific methods. In an article in the same book, following the general direction set by Dewey's work, I suggested that axiology might be regarded as the science of preferential behavior. Since a study of such behavior would of course include the way it affects and is affected by signs, this characterization of axiology embraces the study of preferences (prizings, selection-rejections), appraisals (evaluations, judgments of value), and their interrelations. So conceived, the field of value is open to scientific study.

As examples of studies of prizings in line with this approach reference may be made to the monograph by Henry A. Murray and Christiana D. Morgan, "A Clinical Study of Sentiments";[5] and to the various papers by Jerome S. Bruner and his associates.[6] For some years the speaker has been making a study of the preferences of various kinds of persons in various cultures. While the study has been mainly concerned with preferences among various possible ways to live, some attention has been

given to preferences as to paintings and philosophies, and to the interrelation of these three sets of preferences. It is possible in this way to isolate empirically various value-patterns, to ascertain their number, to study their relative strengths in various cultures, and to investigate what kinds of persons accept which patterns under what conditions. The detailed results of this study have not yet been published, but an indication of the methods and the type of results can be found in an article "Individual Differences and Cultural Patterns" in the volume *Personality,* edited by Clyde Kluckhohn and Henry A. Murray, and in my book, *The Open Self.*[7]

The empirical study of appraisals has been slower in getting under way, since it required a theory of signs sufficiently developed to handle such signs and to compare them with other kinds of signs. It is my belief that the analyses given in *Signs, Language, and Behavior,* in Book III of C. I. Lewis' *An Analysis of Knowledge and Valuation,* and in C. L. Stevenson's *Ethics and Language,* have gone a considerable way toward meeting this need, and hence to prepare the ground both for the scientific study of appraisals and the scientific making of appraisals.

The situation seems to me to be something like this: appraisals are signs the evidence for whose applicability is in the last analysis found in prizings. As such, appraisals are cognitive (true or false) since they make predictions which can be tested; but they differ from certain other kinds of signs whose applicability does not involve the occurrence or non-occurrence of prizings as evidence. It is true, as Stevenson maintains, that conflicts of prizings are not necessarily resolved by the increase of knowledge; it is even true that a certain diet or way of life good with respect to some persons may be bad with respect to others. But these facts force us only to qualify our appraisals with respect to certain persons under certain conditions, and to admit that knowledge of values does not insure their attainment; they do not force us to abandon the view that appraisals are empirically meaningful and capable in principle of being controlled by scientific methods.[8]

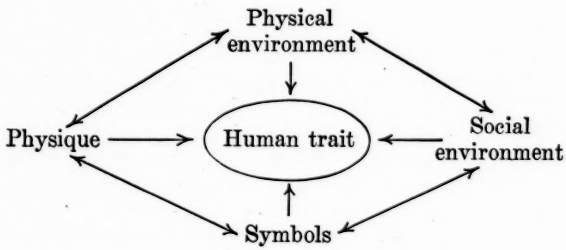
If prizings and appraisals are subject to scientific study, then so is the problem of their relation. It is possible to investigate the influence of certain prizings in the formation or acceptance of appraisals, and the reverse effect upon prizings of appraisals formed by an individual or known to be held by other individuals. Similarly, the relation of prizings and appraisals to non-evaluative knowledge is open to experimental determination. My

study of paintings has touched upon these matters, but serious work on such problems is yet to be done.

It is often maintained that preferences are not values, and that consequently a study of preferences has nothing to do with the study of values. To admit the first part of this thesis does not, however, require an acceptance of the conclusion. It is true that when we ascribe value to something we often mean that it is value-able, i.e., able to support a prizing, or able to support continued prizing, or more able to do either of these than is something else. These are, to be sure, claims made about an object, and not a description of existing prizings. But since they are claims to *prizability,* they can only ultimately be tested by observing whether, under stipulated conditions, the objects are prized or continue to be prized. So without claiming that preferences and values are the same thing, it is possible to maintain that axiology, conceived as the study of preferential behavior, is a study of values. I, indeed, know of no other way that values can be studied.

The Science of Man

We must now note a third growing tendency in the scientific study of man: the tendency to regard any particular human trait as a function of many factors or variables, i.e., to think in terms of fields. This attitude contrasts with the tendency to look at man only in terms of physique or environment or culture or language. Each of these approaches has often been carried on in isolation, and indeed in competition with the other approaches; now we seem to have come to the point where we must realize that any of these approaches when carried far enough involves all of the others.[9] The resulting point of view may be crudely indicated in a diagram. ("Symbols" in this diagram stands for signs produced by an individual and affecting their producer.)



The trait to be studied may be anything whatsoever: clothes worn, philosophies accepted, schizophrenia, aggressiveness, literary preferences, and so forth. In studying this trait in individuals one seeks to find the conditions of the field under which the trait appears. Thus if one is studying schizophrenic traits from this point of view one would try to find out what types of organisms, in what environments, under what social relations, and equipped with what symbols, become schizophrenic. In seeking to uncover such laws, many individuals would have to be studied and many factors considered. The methods of factor analysis developed by L. L. Thurstone (in his *Multiple Factor Analysis*) furnish one important way by which the complex interrelations of such data can be handled.

The field approach to the study of man provides the context in which the science of signs and the science of values are themselves to be carried on. These sciences, conceived as behavioral sciences, are linked in the ways previously indicated with the biological and physical sciences. Putting them within the field approach brings out this relation in another way, for this approach requires that data on physiques and physical environment be included in the study of human traits. The science of man is in this way linked methodologically with the biological and physical sciences to form a single scientific system.

One of the merits of William H. Sheldon's important work (as reported in *The Varieties of Human Physique, The Varieties of Temperament,* and *The Varieties of Delinquent Youth*) is that his constitutional emphasis has forced us to take account of the biological differences in the individuals whose traits are being studied. He has supplied constitutional data on many of my subjects, and at a later time detailed reports will show how this research on psychoses, ways to live, and paintings in various cultures has confirmed his basic thesis as to the importance of considering differences of physique in the study of human beings. Here it is only necessary to add that Sheldon's work is of significance in helping to bridge the gap between the biological and the human sciences by reinserting the physique into the total field. The same service with respect to environmental factors has been performed by Ellsworth Huntington, whose life work was brought to a focus in *Mainsprings of Civilization.*

The constitutional and environmental factors link the science of man with the biological and physical sciences. At the same time, the recognition in personality development of the role of social factors and the role of symbols which individuals produce,

ensures us that nothing distinctively human will be lost. The science of man is linked with the biological and physical sciences but not reduced to them. For as sign processes the ideas and ideals of individuals enter as formative factors in the total field; they are the way in which man in a genuine sense is one factor in the making of his own future, transforming his body, his physical environment, and his culture.[10]

Conclusion

Such are some of the ways in which the work of the last decade has advanced the program of integrating the science of man and bringing it within the framework of unified science. Further and rapid developments in this direction may be expected in the next decade. Such developments will not be of scientific interest alone; they are of importance for the integration of man as man. They help to destroy at the root the opposition of man's scientific and humanistic concerns. In integrating science we at the same time integrate and liberate ourselves.

NOTES

[1] An abstract of this paper was printed as part of the article "The Significance of the Unity of Science Movement," *Philosophy and Phenomenological Research*, 6, 1946, 508-515.

[2] References to criticisms of this approach, and my reply to these criticisms, may be found in "Signs About Signs About Signs," *Philosophy and Phenomenological Research*, 9, 1948, 115-133.

[3] See the papers by Warren S. McCulloch and Walter Pitts, *Bulletin of Mathematical Biophysics*, 5, 1943, 115-133; 9, 1947, 127-147. Also, D. O. Hebb's book, *The Organization of Behavior*.

[4] *International Encyclopedia of Unified Science*, vol. II, no. 4.

[5] *Genetic Psychology Monographs*, 32, 1945, 3-149, and 153-311.

[6] Such as "Value and Need as Organizing Factors in Perception," *J. of Abnormal and Social Psychology*, 42, 1947, 33-44; "Personal Values as Selective Factors in Perception," ibid., 43, 1948, 142-154.

[7] An article, "Comparative Strength of Life-Ideals in Eastern and Western Cultures," is to appear in a volume of proceedings of the second East-West Philosophers' Conference, to be edited by Charles A. Moore. [This volume has since been published under the title *Essays in East-West Philosophy;* Honolulu, University of Hawaii Press, 1951.]

8 This issue is of such basic importance that it deserves to be made a central topic of discussion by the Institute for the Unity of Science. Philipp Frank's position in *Relativity—A Richer Truth* seems in accord with the position taken; but I am not sure that Reichenbach or Stevenson would agree.

9 This point of view provides the basis of organization of the anthology *Personality,* edited by Kluckhohn and Murray. Murray has sometimes called this the "multiform method." Laura Thompson had come to a similar methodological position as a result of her work on the Hopi Indians; see her book *Culture in Crisis.* I had been led to the same result in the study of signs and values. This view facilitates the integration of the science of man, including therein the social sciences. A comprehensive attempt at such integration is found in the forthcoming volume *Toward a General Theory of Action,* by members of the Harvard Department of Social Relations and their collaborators. This book will appear in 1951 or 1952, Harvard University Press, with Talcott Parsons as editor. The point of view is an extension and elaboration of that found in Talcott Parsons' *The Structure of Social Action.*

10 See *The Open Self;* G. H. Mead's *Mind, Self, and Society;* F. S. C. Northrop's "Ideological Man in His Relation to Scientifically Known Natural Man" in *Ideological Differences and World Order,* of which he is the editor.

PART 2

THE "MEANING" OF SCIENTIFIC STATEMENTS

THE VERIFIABILITY THEORY OF MEANING

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I

In 1923 I was asked by the editors of the "Handbuch der Physik" to write an article about the philosophy of physics. Since one of the points I wanted to discuss in this article was the reality of the physical world, I used the occasion of a trip to the Bavarian mountains to see Moritz Schlick at his summer vacation place and to talk with him about this problem. I told him that I had recently discussed these questions with a young philosopher who was writing a book entitled "Der logische Aufbau der Welt", and who advanced the idea that Mach's positivism could be given a strict logical form by regarding any statement about the physical world as logically equivalent to a combination of statements about sense data. I myself objected to this equivalence; I thought that only a probability relationship could be maintained. Schlick supported me in this view; he argued that the positivistic equivalence was untenable.

That was in 1923. A few years later, after Schlick had invited Rudolf Carnap to become a member of his department and had succeeded in attaching Ludwig Wittgenstein to his circle in Vienna, Schlick changed his views completely and turned positivist. In this environment, those views were elaborated which culminate in the thesis that the meaning of a sentence is the method of its verification. Mainly as the achievement of Carnap,[1] the verifiability theory of meaning was thus given an elaborate logical form; for the first time in its long history, which goes back to William of Occam and Leibniz, this theory was set forth with the precision tools of modern symbolic logic.

My article written in 1923 had to wait for publication until 1929, when the volume of the Handbuch finally came out.[2] My refusal to accept the equivalence interpretation was based on the distinction between a complex (Gebilde) which is "artgleich" to its elements from a complex which is "artverschieden" to its elements, a thesis which I took over into a later book of mine as the distinction between a reducible and a projective complex of elements.[3] But I knew that it was vain to go into a further elaboration of my conception before I had been able to con-

struct a theory of probability, since the concept of probability was fundamental to any theory of knowledge. Thus I concentrated my work on the analysis of the calculus of probability and its applications to physical reality, a subject on which I had been working since 1914.

After I had given to the problem of probability and induction a solution[4] which I regarded, and still regard, as satisfactory, I published in 1936 a version of the verifiability theory of meaning[5] which referred to the views of the Vienna circle but differed from it in several points. First, it substituted probability relations for equivalence relations and conceived of verification as a procedure in terms of probabilities rather than in terms of truth. Second, it abandoned the program of defining "the meaning" of a sentence. Instead, it merely laid down two principles of meaning; the first stating the conditions under which a sentence has meaning, the second the conditions under which two sentences have the same meaning. A rule of the latter kind was sufficient for all practical uses of language; and once the equivalence between indirect sentences and observation sentences was abandoned, an explicit definition of the meaning of indirect sentences in observational terms could no longer be given.

Shortly afterwards, Carnap presented the first exposition of his revised views on meaning. He had seen that the equivalence interpretation could not account for the structure of the language of science and proceeded to lay down conditions of meaning rather than give a definition of the meaning of an indirect sentence. This interesting paper on Testability and Meaning[6] marks the beginning of a new period for all those investigations which emanate from the positivism of the Vienna circle. The thesis that the meaning of a sentence is the method of its verification was abandoned. Carnap's reduction chains allot meaning to a term for the case that certain physical conditions occur, without supplying an explicit definition of the term. Thus what I call the first principle of the theory of meaning was here carried through in a form different from mine. It seems to me, though, that Carnap's treatment of meaning calls for a supplementation by some version of my second principle, since otherwise terms which we all would call meaningless could be adjoined to meaningful terms and thus acquire a status in scientific language. For instance, instead of defining the term "there is an electric current in the wire" by means of reduction sentences, we could add to this sentence, following Hempel's example, the invidious sentence "the absolute is perfect", thus including it into the

accepted terms of language. Such adjunction is easily dealt with if a principle is introduced according to which the conjunction of the original with the invidious sentence does not have any more meaning than the original sentence alone.

In his further investigations, Carnap saw that his degree of confirmation, first introduced in the paper mentioned, requires a deeper elaboration through a theory of probability. The views which he has developed in this field differ essentially from mine. I suppose that his theory is intended to overcome the shortcomings of his reduction chains, which are applicable only in cases where probability relations can be practically replaced by logical implications, and which thus are too primitive instruments for the construction of scientific language.

Among former members of the Vienna circle, it was, especially, H. Feigl and C. Hempel who collaborated in adjusting the theory of meaning to a realistic language. Feigl's attempts in this direction go back to the early days of the Vienna circle; he had always been the exponent of realism within this group, and he has recently laid down his views in two excellent articles.[7]

II

After this short historical account, I would like to give a brief analysis of the verifiability theory of meaning in its present status, underlining those points about which there exists agreement among the various adherents of the theory, and setting apart those points about which there is disagreement.

(1) The verifiability theory of meaning lays down rules for the construction of meaningful expressions. These rules are conventions determining the structure of language. Being rules, they are neither true nor false, but volitional decisions. However, it is possible to make cognitive statements about the properties of the language resulting from the acceptance of these rules. These statements have the form of implications: if this convention concerning meaning is accepted, then the language thus resulting has such and such properties. It is possible to study such relations for various definitions of meaning and to compare the various languages. If finally one convention concerning meaning is accepted, it is possible to justify the decision for this set of rules in terms of a certain aim; for instance, the aim of interpreting the language of physics, or the aim of constructing a language that can be used for the purpose of human action.

On this point there is general agreement among the adherents of the verifiability theory of meaning. The problem of meaning falls under the category of what Carnap has called *explication.* A term of conversational language, so far used in a vague sense, is replaced by a precise term. The original term is called *explicandum;* for the term proposed to replace it I will use the name *explicans* (the term "explicatum" used by Carnap is misleading because of its grammatical form, which means *"what is explained",* whereas "explicans" means "what explains"). An explication can never be called true; however, it can be justified. That is, it can be shown that the explicans has properties which make the use of the term compatible with human behavior in connection with it.

(2) The verifiability theory of meaning concerns *cognitive* meaning. It therefore is not concerned with the meaning of imperatives, in which category value judgments are included, though they often do not have the grammatical form of imperatives. It is possible to correlate to imperatives a cognitive statement; for instance, to the imperative "shut the door" the cognitive statement "the speaker wants the door to be shut". Imperatives may therefore be said to have a cognitive component, which is taken care of in the verifiability theory of meaning. This is not meant to say, however, that the question of the meaning of imperatives is completely settled by the transition to the cognitive component. On this point, too, there is, I think, general agreement among the adherents of the verifiability theory of meaning; the theory concerns only cognitive meaning.

(3) Cognitive meaning is defined by a step process. First, a basic class O of *observational sentences* and terms is introduced which are assumed to have *direct meaning.* We may also speak of *primitive meaning,* i.e., a meaning which is not under investigation during the analysis to be performed. Sentences and terms of the class O may also be called *direct sentences,* or *direct terms,* respectively. Second, the meaning of further terms and sentences is constructed by the help of *derivative relationships* D, which connect these new terms with the basis O. A step process of this kind is assumed in all forms of the verifiability theory of meaning. The emphasis on this step process and the analysis of the construction of indirect terms is one of the major contributions of the Vienna circle to the theory of meaning.

(4) The properties of the basic sentences O must be stated more explicitly. It is sometimes said that we must know how to

verify them, and that this is the same as knowing their meaning; in other words, that for the basic sentences at least, the identity of meaning and method of verification can be assumed. But the statement "we must know how to verify them" is not very precise and open to criticism. Suppose a man is given a list of report sentences (Protokollsätze) which are not his own reports, but which he has good reasons to believe to be true. Can the man, whom I will call the reconstructor, construct the system of knowledge resulting from this list of sentences? Obviously it does not suffice him to know that the sentences are true; he must *understand* the sentences. This qualification can be made clear as follows. Suppose the report sentences are written by the use of the logical symbolism. Now if the two sentences "Peter is tall" and "John is tall" are written down in the list, it would not suffice if they are symbolized by the letters "p" and "q". They must be symbolized as "$f(x_1)$" and "$f(x_2)$", since otherwise the usual inferences, for instance, an inference of induction by enumeration, cannot be drawn. This shows that the structure of the report sentences must be known. That is, the report sentences must be symbolized, not in the calculus of propositions, but in the calculus of functions. A further distinction must be added. Even if the report sentences are given in their full structure, the reconstructor does not know to which of his own experiences the symbols "f", "x_1", etc., refer. In spite of the absence of such knowledge, he is able to follow all the inferences made for the construction of scientific theories, and indirect terms thus acquire for him a structural meaning. But what is still missing is the relation to his own experiences. For this purpose, a final step is required. The reconstructor must be given the interpretation of the empirical constants "f", "x_1", etc.; then all terms, including the indirect ones, acquire for him an interpreted meaning. For these reasons, the formulation "the person must know how to verify the sentences of the basis O" is better replaced by the formulation "the person must know the meaning of the sentences of the basis O".[8]

For the basis O, the following kinds of sentences have been used:

(α) reports about the objects of our personal macroscopic environment (concreta) at a certain moment

(β) reports about the same kind of objects, but including our past observations

(γ) reports about the same kind of objects given by any human observer at any time

(δ) reports about sense data at a certain moment (or, as in β or γ, extended to include the past, or different observers)

(ε) reports about atomic physical occurrences, like coincidences of electrons, etc.

As to the choice of these bases, which were discussed at greater detail by Carnap and Neurath, there seems to be general agreement that the basis α has a psychological priority. The objects of the other bases then are regarded as constructed by inferences starting from the basis α. Doubts have been uttered whether the basis α is large enough; in fact, it is difficult to show how on that basis the statement that Caesar was murdered in 44 B.C. can be constructed. It seems we have to include in the basis our recollection of established laws, such as the law that looking into a suitable reference book you find reliable historical data about Caesar; but these recollections are mostly only potentially there. Who would be willing to include the unconscious in the basis α? A clarification of this point is desirable, although I believe that it involves no difficulties of principle.

(5) I will now turn to the derivative relationships *D*. There is today, finally, agreement among most of us that these relations cannot be equivalences between indirect and direct sentences. This widening of meaning is necessary in order to admit as meaningful such sentences as "the planets will go on traveling on their orbits after the death of the last human being".[9] Furthermore, it is agreed that the relations *D* cannot be merely deductive relations from the indirect to the direct sentences. The relations must transfer truth in the opposite direction: the basic sentences must confer a degree of truth-character upon the indirect sentences. This requirement means that the indirect sentences must be so constructed as to be appropriate for the derivation of observable predictions, i.e., of future direct sentences which we have good reason to treat as true before they are directly verified. This cautious formulation will indicate that the derivative relations *D* involve inductive inferences and that such inferences cannot be proved to lead to true conclusions, or even to a large number of true conclusions.

Now we all know that the interpretation of probability and induction is highly controversial today; in fact, the derivative relations mark the point where the controversy about probability

enters into the domain of the verifiability theory of meaning. My own interpretation of probability and induction is so constructed that it satisfies the above requirement; this is admitted, as far as I see, even by my opponents, with the exception of Bertrand Russell, who claims that my inductive posit "is admitted to be not intellectually justified".[10] I would like to know where he found such an admission in my writings. The objections which other writers have raised against my theory of induction and probability maintain that the frequency interpretation of probability is unsuitable for the reason that it does not express what we really mean by a probability referred to a single case, or to a scientific theory. I answer that we are concerned here with an explication, and that for an explication of probability it does not matter what a man means. The explicans given by me has the property of making a man's behavior justifiable whenever he is in a situation which, in correspondence with established usage, he describes by means of the term "probable"; or in other words, it has the property of making the use of the term "probable" compatible with human behavior in connection with the term. This is a sufficient reason to accept my explication.

If others suggest other interpretations, they would have to give a similar proof for the explicans suggested by them. As far as I see, none among the advocates of other interpretations has even tried to give such a proof. Carnap regards his interpretation of probability as justified because, as he says, it is in agreement with the beliefs which a scientist has when he makes inductive inferences.[11] I do not think that this psychological brand of justification is acceptable. Scientists often have strange beliefs, and make fallacious inferences with good results. The logician is not interested in copying the scientist's mistakes. What he wants to give is a rational reconstruction of scientific inference, and for a reconstruction of this kind, correspondence to beliefs is no criterion of adequacy.[12] The only way of showing that a reconstruction of scientific inference is rational is to give a proof that the explicans logically validates the inference. It would be too much to ask that the conclusion be true; the cautious formulation above given is meant to explicate the term "validate". I was glad to see that Carnap now admits that a justification of induction must be given, and that he does not share Schlick's views according to which the justification of induction concerned a pseudo problem. But I cannot regard his interpretation of probability as justified unless he gives a proof that it is advisable, in some sense, to use his probability values as a di-

rective for action.[13] From the presentations of his theory which he has published I would infer that such a logical justification can never be given, because he introduces a probability metric *a priori*.[14] But if the scientist should have to put up with an interpretation which makes his inferences unjustifiable in the logical sense, he could as well quit working.

(6) After defining the basis O and the derivative relations D, the term "verified" can be defined as "being derived from the basis O in terms of the relations D". Now there is general agreement that the condition of meaning is not actual verification, but possible verification. This widening of meaning is necessary in order to admit as meaningful such statements as "it snowed on Manhattan Island in the year 4 A.D.". Therefore the term "possible" must now be defined.

Three kinds of possibility must be investigated: logical, physical, and technical possibility. The first means non-contradictory character; the second, non-contradictory to empirical laws; the third, being within the reach of known practical methods. That a verifiability defined in terms of technical possibility makes the definition of meaning too narrow is now generally admitted. Schlick, and with him most members of the Vienna circle, have used logical possibility. But a definition of meaning in terms of logical possibility of verification makes the definition of meaning too wide, at least, when the interpretation of physics is concerned. For instance, Einstein's principle of equivalence, according to which being in accelerated motion means the same as being in a gravitational field, presupposes a definition of verifiability based on physical possibility. For these reasons I have advocated a definition of meaning in terms of the physical possibility of verification. A suitable definition of this kind of possibility and of physical laws is given in the frame of my theory of nomological statements.[15]

Since meaning is a matter of definition, it must be kept in mind that none of the three definitions resulting from the three kinds of possibility can be called "true"; moreover, none of them seems to supply the only suitable explicans of meaning. It appears that all three of them are actually used. The physicist usually assumes *physical meaning*, for which verification is physically possible. But in his discussions of physical theories which he wants to prove false, he often uses *logical meaning*, for which verification is logically possible. For instance, an absolute time can very well be defined by speaking of the logical possibility of signals faster than light; and when the physicist says that the

theory of absolute time is false, he has assumed logical meaning for it. The simultaneous use of the three definitions of meaning is very expedient for the discussion of physical theories and their comparison under the viewpoint of empirical truth. We may say, however, that for the actual system of physics, physical meaning is generally assumed.[16]

(7) The conditions 1-6 together with condition 8, which I shall deal with presently, lay down conditions of meaningfulness and thus specify what I have called the first principle of the verifiability theory of meaning. The second principle, which defines sameness of meaning, must now be added.

Although equivalence was not assumed to hold between direct and indirect sentences, there can be equivalences between indirect sentences; and it is indispensable for a theory of meaning to lay down the condition that such equivalences lead to sameness of meaning. The three forms of possibility lead to a corresponding division for this category. If the equivalence is analytic, i.e., follows from the rules of logic, we have logical identity of meaning; if the equivalence is synthetic and follows from the laws of physics, we have physical identity of meaning. Technical identity of meaning is a dispensable concept. The above example of Einstein's principle of equivalence illustrates physical identity of meaning. It is an important part of physical research to discover such identities; this has been very well pointed out by Feigl,[17] who mentions the identity of light waves and electromagnetic waves, of heat and average kinetic energy of the molecules, etc., and discusses in this frame the mind-body problem.

As before, it can be useful to employ different definitions of identity of meaning in the same context; for instance, to use Frege's example, we may say that the expressions "the morning star" and "the evening star" have the same physical meaning, but not the same logical meaning. Furthermore, it appears necessary to subdivide the category of logical identity of meaning and to define synonymity by a much narrower requirement, so that, for instance, the terms "five" and "number of regular polyhedrons" are not synonymous, though they can be shown to be tautologically equivalent. A definition of synonymity of this kind was recently given in a highly satisfactory form by Carnap.[18]

It is the practical significance of the second principle of the verifiability theory of meaning that it allows for the translation of expressions of emotive languages into an empirical language.

Statements about mystical visions, or about "moral truth", can thus be given an empirical content, though such a translation may provoke violent opposition on the side of the advocate of superempirical meanings. Visions then acquire the status of dreams, or hallucinations, of certain persons; and "moral truths", the status of beliefs of certain persons. I refer to another publication.[19]

This discussion of identity of meaning answers the objections raised by Bertrand Russell,[20] which are based on earlier versions of the verifiability theory of meaning. He argues that, for the positivist, speaking about other persons' minds would mean the same as speaking about dreamed objects. I doubt whether this is true even for the earliest form of positivism, where the distinction between dream and waking state was already made. And it certainly is not true for a verifiability theory of meaning which allows for indirect verification, since in such a theory identity of meaning does not hold between indirect sentences and observation sentences.

(8) I now come to the discussion of a set of rules which had been overlooked in the earlier discussion of meaning and which were first introduced in my analysis of the problem of unobservables of quantum mechanics.

Going from the sentences of the basis O to indirect sentences is the same as going from observables to unobservables. We are all agreed that this inference is not a matter of belief in a transcendental reality; in fact, if the verifiability theory of meaning is accepted, this would be a belief in something meaningless. So, it must be a form of inductive inference; and thus arises the difficulty of how to make inductions which connect unobservables to observables. Inductions concerning a certain object must start with some properties of this object and then may add further properties to it. But since we have no knowledge of the unobservables, we have nothing to start with. We cannot say: so far the things have existed when we were not looking at them, therefore they will do the same in the future. The premise of this inference is not verified by any observation, and cannot be so verified because of the definition of the term "unobserved object".[21]

This simple analysis shows that statements about unobservables cannot be introduced unless certain conventions are added to our language. In my investigation of quantum mechanics,[22] I have shown that the usual language of science includes the convention that unobservables follow the same physical laws as observables;

in particular, that they satisfy the principle of causality, which for observables is an empirical law. Without such a convention, the language of unobservables is incomplete and not accessible to verification. It leads to statements of the same kind as, for instance, the statement that during the night all things, including our bodies, have become ten times larger. Such paradoxes spring from incompleteness of language, and are easily eliminated by making the language complete through suitable rules.

I will call rules of this kind *extension rules*. They extend the range of laws from observables to unobservables. They are conventions determining the structure of language. Varying the convention, we arrive at a set of *equivalent descriptions,* which are all true to the same extent. Among them, the description for which unobservables follow the same laws as observables is called the *normal system*. Yet the necessity of conventions opens up an empirical investigation: it requires an analysis whether a certain convention can be carried through, i.e., does not lead to contradictory statements about observables. Classical physics can be carried through consistently on the convention that the laws of unobservables are the same as the law of observables. Classical physics, therefore, has a normal system—which is nothing but the usual realistic language. Quantum physics does not admit of a normal system. This is a domain where the mentioned extension rule breaks down and other extension rules have to be used. We have therefore to distinguish between admissible and inadmissible conventions of language. Whether a convention is admissible, is an empirical question. If it is admissible, its use makes the resulting language not "more true" than the use of another admissible convention.

I hope these formulations will clarify a controversy which has arisen about the exposition of realism in my book "Experience and Prediction". I have introduced the term *illata* for objects that can be described only in indirect sentences.[23] This term is as legitimate as the term "indirect sentence". Certainly, illata are not determined unless the extension rules of the language are given; but this fact does not make them "less real". They share this property with all other things, since nothing can be described adequately in an incomplete language. Furthermore, in the discussion of a cubical world[24] in which bird-like shadows are visible, I have argued for a realistic language which speaks about the birds although they are unobservable and thus are illata. In the terminology of my analysis of quantum mechanics, I would say that in this world the realistic language constitutes the normal

system, whereas the positivistic language, which speaks only about the shadows on the walls, is a restrictive interpretation. My argument therefore is that there is no reason to introduce a restrictive interpretation if a normal system exists.[25] The same would apply to quantum mechanics if, for instance, the corpuscle interpretation could be carried through without causal anomalies, even if Heisenberg's indeterminacy were to remain valid and a simultaneous ascertainment of position and momentum were impossible.

When I say that we have inductive evidence for the existence of the external world, I mean the fact that the realistic language can be carried through for the macrocosm, that there is a normal system. This is a verifiable and meaningful statement; but of course, it is so only after the language is made complete through extension rules. That the realistic hypothesis is empirical, and not a faith, or perhaps a truism, is shown by the strange results of quantum mechanics, which exclude the realistic hypothesis, in the usual sense, for the microcosm. Here we can speak about physical reality only in an indirect way, using the duality of wave and particle description; instead of the normal system, we use a set of equivalent descriptions, none of which has the directness of the normal system. A realistic language in the sense of the language of the macrocosm is here impossible; if there is any realism left, it is certainly not a "naive realism".

I should like to add the remark that the reality problem of quantum mechanics has been construed as resulting from the influence of the human observer and thus as confirming idealistic philosophies, according to which the *ego* creates the external world; in another version of such ideas, it has been argued that the line of demarcation between observer and external object cannot be clearly drawn. I do not think such a "metaphysical" interpretation of quantum mechanics is tenable. The observables of quantum mechanics are not human perceptions, but physical occurrences of a certain kind; and the quantum mechanical difficulties begin when the realm of these observables, or phenomena, is to be supplemented by a realm of unobservables, or interphenomena. It is therefore not the step from the human observer to the external object, but the step from macrophysics to microphysics which involves the difficulties. The quantum-mechanical indeterminacy is an entirely physical affair; i.e., it expresses a structural property of the physical world, which has nothing to do with the fact that this world is observed by human beings.[26] That the indeterminacy influences our system of knowl-

edge has other reasons. Our bodies are macroscopic organisms, and our direct observations are restricted to macroscopic objects. Our knowledge of the microcosm is acquired by way of the macrocosm, and on this transition the indeterminacy enters our statements about the microcosm.

In order to construct an adequate realistic language for quantum phenomena I have suggested to use a three-valued logic, which possesses a category of indeterminate sentences between those of true and of false sentences. Since the indeterminate sentences are neither true nor false, the objection has been raised that my interpretation of quantum mechanics contradicts the verifiability criterion of meaning.[27] This is a misunderstanding. The theory of meaning has been emancipated, for a long while, from its first dogmatic version and has assumed a moderate version, which admits of modified forms of verification. In order to include the three-valued logic of quantum mechanics, the conditions of meaningfulness, in particular, the extension rules, have to be widened so as to admit the category of indeterminate statements. Since these statements are connected by well-defined rules with the observational basis of language, they are as legitimate as any other statements exempt from direct verification. To put it briefly: a quantum-mechanical statement is meaningful if it is verifiable (in the moderate sense of probability verification) as true, false, or indeterminate.

III.

This is the state of the verifiability theory of meaning in 1950. From 1923 to 1950 there has been a development abundant in results. It sprang from the cooperation of men, and of groups of men. The Vienna circle and the Berlin group, in spite of all their differences, remained friends and worked together; and their work was continued in the wider group of logical empiricism, which grew out of their alliance and includes men from many countries, among them many from this country and many who have made this country their second home. The progressive development of their views is reflected in the development of their theory of meaning. What was guesswork and program in the beginning, has been subject to methods of logical analysis and has acquired the status of an elaborate logical theory. It could go through this development only because the earlier versions of this theory were formulated and defended with the ardor and stubbornness of the philosopher who thinks to have found the truth; and because a better truth is strong enough to overcome

the stubbornness of the philosopher and to kindle a new ardor for better formulation. Let us be aware that, if not everything, yet philosophy is in flux; and that those who discover new results learn by their mistakes. The method of trial and error is not only the tool of the experimental scientist; it is also the tool of the philosopher.

NOTES

[1] R. Carnap, Der logische Aufbau der Welt, Berlin, 1928.

[2] "Ziele und Wege der physikalischen Erkenntnis", in H. Geiger and K. Scheel, *Handbuch der Physik*, Vol. IV, 1929, pp. 1-80. See pp. 16-24.

[3] Experience and Prediction, Chicago 1938, p. 111, p. 130.

[4] Wahrscheinlichkeitslehre, Leiden 1935.

[5] Logistic Empiricism in Germany, *Journ. of Philosophy*, vol. 33, 1936, pp. 147-150, p. 154.

[6] R. Carnap, Testability and Meaning, *Philosophy of Science*, vol. 4, 1936, p. 419, and vol. 5, 1937, p. 1.

[7] H. Feigl, Existential Hypotheses, *Philosophy of Science*, vol. 17, 1950, p. 35; The Mind-Body Problem, *Revue Internationale de Philosophie*, No. 11, 1950, p. 64. The same issue of the *Revue* contains interesting articles by Carnap and Hempel.

[8] I think these remarks correspond to views about the verifiability theory of meaning recently presented by P. Marhenke, Presidential Address to the Pacific Division of the American Philosophical Association, Christmas 1949, to be published in *Philosophical Review*.

[9] See the discussion of such sentences in my book "Experience and Prediction", Chicago 1938, pp. 133-135.

[10] Bertrand Russell, Logical Positivism, *Revue Internationale de Philosophie*, No. 11, 1950, p. 19.

[11] R. Carnap, On Inductive Logic, *Philosophy of Science*, vol. 12, 1945, p. 95.

[12] I would therefore say that for a fallacious argument, as a whole, there is no rational reconstruction. Such argument can be analyzed as follows: the argument contains a "jump" from one point P_1 to another point P_2. From the premises up to P_1, and from P_2 to the conclusion, we can give a rational reconstruction; but for the transition from P_1 to P_2, there is no rational reconstruction. It seems to me that even the term "reconstruction"

cannot be defined without some criterion of validity, and that fallacious thinking cannot be reconstructed unless the reconstruction is rational at least with respect to parts of the argument.

[13] Carnap's paper, "Probability as a Guide in Life", *Journ. of Philos.*, vol. 44, 1947, p. 141, does not contain any proof of this kind.

[14] I refer to the discussion in my book "The Theory of Probability", Berkeley 1949, §71 and §88.

[15] Elements of Symbolic Logic, New York 1947, chap. VIII.

[16] It is a pity, therefore, that Carnap in his enlightening book "Meaning and Necessity", Chicago 1947, does not speak at all about physical meaning and physical possibility.

[17] H. Feigl, The Mind-Body Problem, *Revue Internationale de Philosophie*, No. 11, 1950, p. 80.

[18] R. Carnap, Meaning and Necessity, Chicago 1947, §§14-15. Incidentally, it seems to me that the outcome of this book can be summarized in the following thesis: the major point is the definition of the terms "having the same extension" and "having the same intension"; interpretations of the terms "extension" and "intension" can then be added more or less arbitrarily.

[19] Experience and Prediction, Chicago 1938, pp. 66-68.

[20] Bertrand Russell, Human Knowledge, New York 1948, p. 449.

[21] This argument was constructed by W. T. Stace, Refutation of Realism, *Mind* 43, 1934. It is also discussed in my book "Philosophic Foundations of Quantum Mechanics," Berkeley 1944, p. 18. It is shown there that the solution consists, not in a refutation of realism, but in a reformulation of realism.

[22] See p. 19 of my book mentioned in the preceding footnote.

[23] Experience and Prediction, Chicago 1938, p. 212.

[24] Ibid. §14.

[25] This answers certain objections by H. Feigl, Existential Hypotheses, *Philos. of Science*, vol. 17, 1950, p. 54, who unfortunately overlooks the exposition of the problem in my book "Philosophic Foundations of Quantum Mechanics" and bases his critique merely on my book "Experience and Prediction".

[26] See my book, "Philosophic Foundations of Quantum Mechanics", p. 15. An imaginary macroscopic analogy of quantum mechanics is described there on pp. 38-39.

[27] E. Nagel, *Journ. of Philosophy*, vol. 42, 1945, pp. 437-444. See also my reply, vol. 43, 1946, p. 244.

THE CONCEPT OF COGNITIVE SIGNIFICANCE: A RECONSIDERATION

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1. THE GENERAL EMPIRICIST CONCEPTION OF COGNITIVE AND OF EMPIRICAL SIGNIFICANCE

It is a basic principle of contemporary empiricism that a sentence makes a cognitively significant assertion, and thus can be said to be either true or false, if and only if either (1) it is analytic or contradictory—in which case it is said to have purely logical meaning or significance—or else (2) it is capable, at least potentially, of test by experiential evidence—in which case it is said to have empirical meaning or significance. The general epistemological orientation expressed in this principle, and especially in its second part, the so-called testability criterion of empirical meaning, is not peculiar to empiricism alone: it is characteristic also of contemporary operationism, and in a sense of pragmatism as well; for the pragmatist maxim that a difference must make a difference to be a difference may well be construed as insisting that a verbal difference between two sentences must make a difference in experiential implications if it is to reflect a difference in meaning.

How this general conception of cognitively significant discourse led to the rejection, as devoid of logical and empirical meaning, of various formulations in speculative metaphysics, and even of certain hypotheses offered within empirical science, is too well known to require recounting. I think that the general intent of the empiricist criterion of meaning is basically sound, and that notwithstanding much oversimplification in its use, its critical application has been, on the whole, enlightening and salutary. I feel less confident, however, about the possibility of restating the general idea in the form of precise and general criteria which establish sharp dividing lines (a) between statements of purely logical and statements of empirical significance, and (b) between those sentences which do have cognitive significance and those which do not.

In the present paper, I propose to reconsider these distinctions as conceived in recent empiricism, and to point out some of the difficulties they seem to present. The discussion will concern

mainly the second of the two distinctions; in regard to the first, I shall have to limit myself to a few brief remarks.

2. The Earlier "Testability" Criteria of Meaning and Their Shortcomings

Let us note first that any general criterion of cognitive significance will have to meet certain requirements if it is to be at all acceptable. Of these, we note one, which we shall consider here as expressing a necessary, though by no means sufficient, "*condition of adequacy*" for criteria of cognitive significance.

(A) If under a given criterion of cognitive significance, a sentence N is non-significant, then so must be all truth-functional compound sentences in which N occurs non-vacuously as a component. For if N cannot be significantly assigned a truth value, then it is impossible to assign truth values to the compound sentences containing N; hence, they should be qualified as non-significant as well.

We note two corollaries of requirement (A):

(A1) If under a given criterion of cognitive significance, a sentence S is non-significant, then so must be its denial, $\sim S$.

(A2) If under a given criterion of cognitive significance, a sentence N is non-significant, then so must be any conjunction $N.S$ and any alternation $N \vee S$, no matter whether S is significant under the given criterion or not.

We now turn to the initial attempts made in recent empiricism to establish general criteria of cognitive significance. Those attempts were governed by the consideration that a sentence, to make an empirical assertion, must be capable of being borne out by, or conflicting with, phenomena which are potentially capable of being directly observed. Sentences describing such potentially observable phenomena—no matter whether the latter do actually occur or not—may be called observation sentences. More specifically, an *observation sentence* might be construed as a sentence—no matter whether true or false—which ascribes to some specified object, or group of objects, of macroscopic size, some particular *observable characteristic,* i.e., a characteristic whose presence or absence can, under favorable circumstances, be ascertained by direct observation.

The task of setting up criteria of empirical significance is thus transformed into the problem of characterizing in a precise manner the relationship which obtains between a hypothesis and one

or more observation sentences whenever the phenomena described by the latter either confirm or disconfirm the hypothesis in question. The ability of a given sentence to enter into that relationship to some set of observation sentences would then characterize its testability-in-principle, and thus its empirical significance. Let us now briefly examine the major attempts that have been made to obtain criteria of significance in this manner.[1]

(2.1) One of the earliest criteria is expressed in the so-called *verifiability requirement*. According to it, a sentence is empirically significant if and only if it is non-analytic and is capable of complete verification by, i.e., logical deduction from, some finite logically consistent set of observation sentences. These observation sentences need not be true, for what the criterion is to explicate is testability by "potentially observable phenomena", or testability "in principle".

In accordance with the general conception of cognitive significance outlined earlier, a sentence will now be classified as cognitively significant if either it is analytic or contradictory, or it satisfies the verifiability requirement.

The criterion thus obtained, however, is unacceptable for several reasons, of which we mention here the following:

a. Let S be a sentence which does, and N a sentence which does not satisfy the verifiability requirement. Then S is deducible from some set of observation sentences; hence, by a familiar rule of logic, $S \vee N$ is deducible from the same set, and therefore cognitively significant according to our criterion. This violates condition (A2) above.

b. Let us assume that the properties of being a stork and of being red-legged are both observable characteristics, and that the former does not logically entail the latter. Then the sentence

($S1$) All storks are red-legged

is neither analytic nor contradictory; and clearly, it is not deducible from a finite set of observation sentences. Hence, under the contemplated criterion, $S1$ is devoid of empirical significance; and so are all other sentences purporting to express universal regularities or general laws. This is in itself a highly objectionable consequence of our criterion. But furthermore, it is readily seen that the denial of $S1$

($\sim S1$) There exists at least one stork that is not red-legged

is deducible from any two observation sentences of the type 'a is a stork' and 'a is not red-legged'. Hence, $\sim S1$ is cognitively sig-

nificant under our criterion, and this constitutes a violation of condition (A1).

(2.2) A second criterion, the so-called *falsifiability requirement,* qualifies a sentence S as significant if S is not contradictory and is capable of complete falsification (refutation) by some consistent finite set of observation sentences.

But this requirement is tantamount to the condition that $\sim S$ satisfy the verifiability requirement; therefore, the new criterion, too, violates (A1) and (A2).

(2.3) A more complex criterion has been proposed by Ayer;[2] it is rather involved, and we shall not state it here, for it can be shown that it violates (A2) by qualifying as significant any alternation with at least one significant component. Furthermore, as has been shown by Church (1949), Ayer's criterion has the unacceptable consequence that if there are three observation sentences none of which entails either of the others, then, if S is any sentence whatever, either S or $\sim S$ is significant under the criterion.

(2.4) An alternative criterion recently suggested by O'Connor (1950) as a revision of Ayer's formulation is subject to a slight variant of Church's stricture: It can be shown that if there are three observation sentences none of which entails any of the others, and if S is any non-compound sentence, then either S or $\sim S$ is significant under O'Connor's criterion.

All the criteria considered so far attempt to explicate the concept of empirical significance by specifying certain logical connections which must obtain between a significant sentence and suitable observation sentences. It seems now that this type of approach offers little hope for the attainment of precise criteria of meaningfulness: this conclusion is suggested by the preceding survey of some representative attempts, and it receives additional support from certain further considerations, some of which will be presented in the following sections.

3. Characterization of Significant Sentences by Criteria for Their Constituent Terms

An alternative procedure suggests itself which again seems to reflect well the general viewpoint of empiricism: It might be possible to characterize cognitively significant sentences by certain conditions which their constituent terms have to satisfy.

Specifically, it would seem reasonable to say that all extra-logical terms[3] in a significant sentence must have experiential reference, and that therefore their meanings must be capable of explication by reference to observables exclusively.[4] In order to exhibit certain analogies between this approach and the previous one, we adopt the following terminological conventions:

Any term that may occur in a cognitively significant sentence will be called a *cognitively significant term.* Furthermore, we shall understand by an *observation term* any term which either (a) is an *observation predicate,* i.e., signifies some observable characteristic (as do the terms 'blue', 'warm', 'soft', 'coincident with', 'of greater apparent brightness than'), or (b) names some physical object of macroscopic size (as do the terms 'the needle of this instrument', 'the Moon', 'Krakatao Volcano', 'Greenwich, England', 'Julius Caesar').

Now while the testability criteria of meaning aimed at characterizing the cognitively significant sentences by means of certain inferential connections in which they must stand to some observation sentences, the alternative approach under consideration would instead try to specify the vocabulary that may be used in forming significant sentences. This vocabulary, the class of significant terms, would be characterized by the condition that each of its elements is either a logical term or else a term with empirical significance; in the latter case, it has to stand in certain definitional or explicative connections to some observation terms. This approach certainly avoids any violations of our earlier conditions of adequacy. Thus, e.g., if S is a significant sentence, i.e., contains cognitively significant terms only, then so is its denial, since the denial sign, and its verbal equivalents, belong to the vocabulary of logic and are thus significant. Again, if N is a sentence containing a non-significant term, then so is any compound sentence which contains N.

But this is not sufficient, of course. Rather, we shall now have to consider a crucial question analogous to that raised by the previous approach: Precisely how are the logical connections between empirically significant terms and observation terms to be construed if an adequate criterion of cognitive significance is to result? Let us consider some possibilities.

(3.1) The simplest criterion that suggests itself might be called the *requirement of definability.* It would demand that any term with empirical significance must be explicitly definable by means of observation terms.

This criterion would seem to accord well with the maxim of operationism that all significant terms of empirical science must be introduced by operational definitions. However, the requirement of definability is vastly too restrictive to be satisfactory, for many important terms of scientific and even pre-scientific discourse cannot be explicitly defined by means of observation terms.

In fact, as Carnap[5] has pointed out, an attempt to provide explicit definitions in terms of observables encounters serious difficulties as soon as disposition terms, such as 'soluble', 'malleable', 'electric conductor', etc., have to be accounted for; and many of these occur even on the pre-scientific level of discourse. These difficulties have been expounded so often[6] that we shall not restate them here, except for saying that they seem to be capable of resolution only by means of a suitable theory of modal connectives or by the use of methods other than explicit definition for the introduction of certain empirically significant terms. A fruitful method of the latter kind has been developed by Carnap.[7] It consists of introducing new terms by means of so-called *reduction sentences;* these are partial, or conditional, definitions of a certain type which includes explicit definition as a special subclass. Reduction sentences provide a satisfactory interpretation of the experiential import of a large class of disposition terms and permit a more adequate formulation of so-called operational definitions, which, in general, are not complete definitions at all. These successes suggested a greatly liberalized alternative to the requirement of definability:

(3.2) *The requirement of reducibility.* Every term with empirical significance must be capable of introduction, on the basis of observation terms, through chains of reduction sentences.

This requirement is characteristic of the liberalized versions of positivism and physicalism which, since about 1936, have superseded the older, overly narrow conception of a full definability of all terms of empirical science by means of observables,[8] and it avoids many of the shortcomings of the latter. Yet, reduction sentences do not seem to offer an adequate means for the introduction of the central terms of advanced scientific theories, often referred to as theoretical constructs. This is indicated by the following considerations: A chain of reduction sentences provides a necessary and a sufficient condition for the applicability of the term it introduces. (When the two conditions coincide, the chain is tantamount to an

explicit definition.) But now take, for example, the concept of length as used in classical physical theory. Here, the length in centimeters of the distance between two points may assume any positive real number as its value; yet it is clearly impossible to formulate, by means of observation terms, a sufficient condition for the applicability of such expressions as 'having a length of $\sqrt{2}$ cm' and 'having a length of $\sqrt{2} + 10^{-100}$ cm'; for such conditions would provide a possibility for discriminating, in observational terms, between two lengths which differ by only 10^{-100} cm.

It would be ill-advised to argue that for this reason, we ought to permit only such values of the magnitude, length, as permit the statement of sufficient conditions in terms of observables. For this would rule out, among others,[9] all irrational numbers and would prevent us from assigning, to the diagonal of a square with sides of length 1, the length $\sqrt{2}$, which is required by Euclidean geometry. Hence, the principles of Euclidean geometry would not be universally applicable in physics. Similarly, the principles of the calculus would become inapplicable, and the system of scientific theory as we know it today would be reduced to a clumsy, unmanageable torso. This, then, is no way of meeting the difficulty. Rather, we shall have to analyze more closely the function of constructs in scientific theories, with a view to obtaining through such an analysis a more adequate characterization of cognitively significant terms.

Theoretical constructs occur in the formulation of scientific theories. These may be conceived of, in their advanced stages, as being stated in the form of deductively developed axiomatized systems. Classical mechanics, or Euclidean or some Non-Euclidean form of geometry in physical interpretation, present examples of such systems. The extra-logical terms used in a theory of this kind may be divided, in familiar manner, into primitive or basic terms, which are not defined within the theory, and defined terms, which are explicitly defined by means of the primitives. Thus, e.g., in Hilbert's axiomatization of Euclidean geometry,[9a] the terms 'point', 'straight line', 'between', are among the primitives, while 'line segment', 'angle', 'triangle', 'length' are among the defined terms. The basic and the defined terms together with the terms of logic constitute the vocabulary out of which all the sentences of the theory are constructed. The latter are divided, in an axiomatic presentation, into primitive statements (also called postulates or basic statements) which, in the theory, are not derived from any other statements, and de-

rived ones, which are obtained by logical deduction from the primitive statements.

From its primitive terms and sentences, an axiomatized theory can be developed by means of purely formal principles of definition and deduction, without any consideration of the empirical significance of its extra-logical terms. Indeed, this is the standard procedure employed in the axiomatic development of uninterpreted mathematical theories such as those of abstract groups or rings or lattices, or any form of pure (i.e., non-interpreted) geometry.

However, a deductively developed system of this sort can constitute a scientific theory only if it has received an empirical interpretation[10] which renders it relevant to the phenomena of our experience. Such interpretation is given by assigning a meaning, in terms of observables, to certain terms or sentences of the formalized theory. Frequently, an interpretation is given not for the primitive terms or statements of the theory, but rather for some of the terms definable by means of the primitives, or for some of the sentences deducible from the postulates.[11] Furthermore, interpretation may amount to only a partial assignment of meaning. Thus, e.g., the rules for the measurement of length by means of a standard rod may be considered as providing a *partial* empirical interpretation for the term 'the length, in centimeters, of interval *i*', or alternatively, for some sentences of the form 'the length of interval *i* is *r* centimeters'. For indeed, the method is applicable only to intervals of a certain medium size, and even for the latter it does not constitute a full interpretation — for at least two different reasons: First, as was noted before, no sentence expressible in observation terms and logical terms alone can state a sufficient condition (let alone a necessary and sufficient one) for a sentence of the form 'the length of interval *i* is *r* centimeters'; and secondly, the use of a standard rod does not constitute the only way of determining length. Various alternative procedures are available involving the measurement of other magnitudes which are connected, by general laws, with the length that is to be determined.

This last observation, concerning the possibility of an indirect measurement of length by virtue of certain laws, suggests an important reminder: It is not correct to speak, as it is often done, of "the experiential meaning" of a term or a sentence in isolation. In the language of science, and for similar reasons even in pre-scientific discourse, a single statement usually has

no experiential implications. A single sentence in a scientific theory, for example, does not, as a rule, entail any observation sentences; consequences asserting the occurrence of certain observable phenomena can be derived from it only by conjoining it with a set of other, subsidiary, hypotheses. Of the latter, some will usually be observation sentences, others will be previously accepted theoretical statements. Thus, e.g., the relativistic theory of the deflection of light rays in the gravitational field of the sun entails assertions about observable phenomena only if it is conjoined with a considerable body of astronomical and optical theory as well as a large number of specific statements about the instruments used in those observations of solar eclipses which serve to test the hypothesis in question.

Hence, the phrase, 'the empirical meaning of expression *E*' is elliptical in character: What a given expression "means" in regard to potential empirical data is relative to two factors, namely:

I. *the linguistic framework L* to which the expression belongs. Its rules determine, in particular, what sentences — observational or otherwise — may be inferred from a given statement or class of statements;

II. the theoretical context in which the expression occurs, i.e., the class of those statements in *L* which are available as subsidiary hypotheses.

Thus, to mention just one more example, the sentence formulating Newton's law of gravitation has no experiential meaning by itself; but when used in a language whose logical apparatus permits the development of the calculus, and when combined with a suitable system of other hypotheses — including sentences which connect some of the theoretical terms with observation terms and thus establish a partial interpretation — then it has a bearing on observable phenomena in a large variety of fields. — Analogous considerations are applicable to the term 'gravitational field', for example. It can be considered as having experiential meaning only within the context of a theory, which must be at least partially interpreted; and the experiential meaning of the term — as expressed, say, in the form of operational criteria for its application — will depend again on the theoretical system at hand, and on the logical characteristics of the language within which it is formulated.

4. Cognitive Significance as a Characteristic of Interpreted Systems

The preceding considerations point to the conclusion that a satisfactory criterion of cognitive significance cannot be reached through the second avenue of approach here considered, namely by means of specific requirements for the terms which make up significant sentences. This result accords with a general characteristic of scientific (and, in principle, even pre-scientific) theorizing: Theory formation and concept formation go hand in hand; neither can be carried on successfully in isolation from the other.

If, therefore, cognitive significance can be attributed to anything, then only to entire theoretical systems formulated in a language with a well-determined structure. And the decisive mark of cognitive significance in such a system appears to be the existence of an interpretation for it in terms of observables. Such an interpretation might be formulated, for example, by means of conditional or biconditional sentences connecting non-observational terms of the system with observation terms in the given language; the latter as well as the connecting sentences may or may not belong to the theoretical system.

But the requirement of partial interpretation is extremely liberal; it is satisfied, for example, by the system consisting of contemporary physical theory combined with some set of principles of speculative metaphysics, even if the latter have no empirical interpretation at all. Within the total system, these metaphysical principles play the rôle of what K. Reach and also O. Neurath liked to call *isolated sentences:* They are neither purely formal truths or falsehoods, demonstrable or refutable by means of the logical rules of the given language system; nor do they have any experiential bearing; i.e., their omission from the theoretical system would have no effect on its explanatory and predictive power in regard to potentially observable phenomena (i.e., the kind of phenomena described by observation sentences).[12] Should we not, therefore, require that a cognitively significant system contain no isolated sentences? The following criterion suggests itself:

(4.1) A theoretical system is cognitively significant if and only if it is partially interpreted to at least such an extent that none of its primitive sentences is isolated.

But this requirement may bar from a theoretical system certain sentences which might well be viewed as permissible and indeed

desirable. By way of a simple illustration, let us assume that our theoretical system T contains the primitive sentence

$$(S1) \qquad (x)\big(P_1x \supset (Qx \equiv P_2x)\big)$$

where 'P_1' and 'P_2' are observation predicates in the given language L, while 'Q' functions in T somewhat in the manner of a theoretical construct: it occurs in only one primitive sentence of T, namely $S1$. Now $S1$ is not a truth or falsehood of formal logic; and furthermore, if $S1$ is omitted from the set of primitive sentences of T, then the resulting system, T', possesses exactly the same systematic, i.e., explanatory and predictive, power as T. Our contemplated criterion would therefore qualify $S1$ as an isolated sentence which has to be eliminated — excised by means of Occam's razor, as it were — if the theoretical system at hand is to be cognitively significant.

But this may be an overly strict procedure; indeed, it is possible to take a much more favorable view of $S1$ by treating it as a partial definition for the theoretical term 'Q'. Thus conceived, $S1$ specifies that in all cases where the observable characteristic P_1 is present, 'Q' is applicable if and only if the observable characteristic P_2 is present as well. In fact, $S1$ is an instance of those partial, or conditional, definitions which Carnap calls bilateral reduction sentences. These sentences are explicitly qualified by Carnap as analytic (though not, of course, as truths of formal logic), essentially on the ground that all their consequences which are expressible by means of observation predicates (and logical terms) alone are truths of formal logic.[13]

Let us pursue this line of thought a little further. This will lead us to some observations on analytic sentences and then back to the question of the adequacy of (4.1).

Suppose that we add to our system T the further sentence

$$(S2) \qquad (x)\big(P_3x \supset (Qx \equiv P_4x)\big)$$

where 'P_3', 'P_4' are additional observation predicates. Then, on the view that "every bilateral reduction sentence is analytic",[14] $S2$ would be analytic as well as $S1$. Yet, the two sentences taken jointly entail non-analytic consequences which are expressible in terms of observation predicates alone, such as[15]

$$(O) \qquad (x)\big(\sim(P_1x \cdot P_2x \cdot P_3x \cdot \sim P_4x) \cdot \sim(P_1x \cdot \sim P_2x \cdot P_3x \cdot P_4x)\big)$$

But one would hardly want to admit the consequence that the conjunction of two analytic sentences may be synthetic. Hence if the concept of analyticity can be applied at all to the sentences of interpreted deductive systems, then it will have to be relativized with respect to the theoretical context at hand. Thus, e.g., $S1$ might be qualified as analytic relatively to the system T, whose remaining postulates do not contain the term 'Q', but as synthetic relatively to the system T enriched by $S2$. — Strictly speaking, the concept of analyticity has to be relativized also in regard to the rules of the language at hand, for the latter determine what observational or other consequences are entailed by a given sentence. This need for at least a twofold relativization of the concept of analyticity was almost to be expected in view of those considerations which required the same twofold relativization for the concept of experiential meaning of a sentence.

If, on the other hand, we decide not to permit $S1$ in the rôle of a partial definition and instead reject it as an isolated sentence, then we are led to an analogous conclusion: Whether a sentence is isolated or not is relative to the linguistic frame and to the theoretical context at hand: While $S1$ is isolated relatively to T (and the language in which both are formulated), it acquires definite experiential implications when T is enlarged by $S2$.

Thus we find, on the level of interpreted theoretical systems, a peculiar rapprochement, and partial fusion, of some of the problems pertaining to the concepts of cognitive significance and of analyticity: Both concepts need to be relativized; and a large class of sentences may be viewed, apparently with equal right, as analytic in a given context, or as isolated, or non-significant, in respect to it.

In addition to barring, as isolated in a given context, certain sentences which could just as well be construed as partial definitions, the criterion (4.1) has another serious defect: Of two logically equivalent formulations of a theoretical system it may qualify one as significant while barring the other as containing an isolated sentence among its primitives. For assume that a certain theoretical system $T1$ contains, among its primitive sentences S', S'', . . . exactly one, S', which is isolated. Then $T1$ is not significant under (4.1). But now consider the theoretical system $T2$ obtained from $T1$ by replacing the two first primitive sentences, S', S'', by one, namely their conjunction. Then, under our assumptions, none of the primitive sentences of $T2$ is isolated, and $T2$, though equivalent to $T1$, is qualified as significant by

(4.1). In order to do justice to the intent of (4.1), we would therefore have to lay down the following stricter requirement:

(4.2) A theoretical system is cognitively significant if, and only if, it is partially interpreted to such an extent that in no system equivalent to it at least one primitive sentence is isolated.

Let us apply this requirement to some theoretical system whose postulates include the two sentences $S1$ and $S2$ considered before, and whose other postulates do not contain 'Q' at all. Since the sentences $S1$ and $S2$ together entail the sentence O, the set consisting of $S1$ and $S2$ is logically equivalent to the set consisting of $S1$, $S2$, and O. Hence, if we replace the former set by the latter, we obtain a theoretical system equivalent to the given one. In this new system, both $S1$ and $S2$ are isolated since, as can be shown, their removal does not affect the explanatory and predictive power of the system in reference to observable phenomena. To put it intuitively, the systematic power of $S1$ and $S2$ is the same as that of O. Hence, the original system is disqualified by (4.2). From the viewpoint of a strictly sensationalist positivism as perhaps envisaged by Mach, this result might be hailed as a sound repudiation of theories making reference to fictitious entities, and as a strict insistence on theories couched exclusively in term of observables. But from a contemporary vantage point, we shall have to say that such a procedure overlooks or misjudges the important function of constructs in scientific theory: The history of scientific endeavor shows with increasing clarity that if we wish to arrive at precise, comprehensive, and well-confirmed general laws, we have to rise above the level of direct observation. The phenomena directly accessible to our experience are not connected by general laws of great scope and rigor. Theoretical constructs are needed for the formulation of such higher-level laws. One of the most important functions of a well-chosen construct is its potential ability to serve as a constituent in ever new general connections that may be discovered; and to such connections we would blind ourselves if we insisted on banning from scientific theories all those terms and sentences which could be "dispensed with" in the sense indicated in (4.2). In following such a narrowly phenomenalistic or positivistic course, we would deprive ourselves of the tremendous fertility of theoretical constructs, and we would also often render the formal structure of the expurgated theory clumsy and inefficient.

Criterion (4.2), then, must be abandoned, and considerations such as those outlined in this paper seem to lend strong support

to the conjecture that no adequate alternative to it can be found; i.e., that it is not possible to formulate general and precise criteria which would separate those partially interpreted systems whose isolated sentences might be said to have a significant function from those in which the isolated sentences are, so to speak, mere useless appendages.

We concluded earlier that cognitive significance in the sense intended by recent empiricism and operationism can at best be attributed to sentences forming a theoretical system, and perhaps rather to such systems as wholes. Now, rather than trying to replace (4.2) by some alternative, we will have to recognize further that cognitive significance in a system is a matter of degree: Significant systems range from those whose entire extra-logical vocabulary consists of observation terms, through theories whose formulation relies heavily on theoretical constructs, on to systems with hardly any bearing on potential empirical findings. Instead of dichotomizing this array into significant and non-significant systems, it would seem less arbitrary and more promising to appraise or compare different theoretical systems in regard to such characteristics as these:

a. the clarity and precision with which the theories are formulated, and with which the logical relationships of their elements to each other and to expressions couched in observational terms have been made explicit;
b. the systematic, i.e., explanatory and predictive, power of the systems in regard to observable phenomena;
c. the formal simplicity of the theoretical system with which a certain systematic power is attained;
d. the extent to which the theories have been confirmed by experiential evidence.

Many of the speculative philosophical approaches to cosmology, biology, or history, for example, would make a poor showing on practically all of these counts and would thus prove no matches to available rival theories, or would be recognized as so unpromising as not to warrant further study or development.

If the procedure here suggested is to be carried out in detail, so as to become applicable also in less obvious cases, then it will be necessary, of course, to develop general standards, and theories pertaining to them, for the appraisal and comparison of theoretical systems in the various respects just mentioned. To what extent this can be done with rigor and precision cannot well be judged in advance. In recent years, a considerable amount of

work has been done towards a definition and theory of the concept of degree of confirmation, or logical probability, of a theoretical system,[16] and several contributions have been made towards the clarification of some of the other ideas referred to above.[17] The continuation of this research represents a challenge for further constructive work in the logical and methodological analysis of scientific knowledge.

NOTES

1 For a more detailed survey, see Hempel (1950). The present study is intended to extend the analysis presented in that earlier article.

2 Ayer (1946), p. 13. This criterion is suggested by Ayer as a correction of an earlier, inadequate one.

3 An extra-logical term is one that does not belong to the specific vocabulary of logic. The following phrases, and those definable by means of them, are typical examples of logical terms: 'not', 'and', 'or', 'if . . . then', 'all', 'some', '. . . is an element of class - - -'. Whether it is possible to make a sharp theoretical distinction between logical and extra-logical terms is a controversial issue related to the problem of discriminating between analytic and synthetic sentences. For the purpose at hand, we may simply assume that the logical vocabulary is given by enumeration.

4 For a detailed exposition and critical discussion of this idea, see H. Feigl's stimulating and enlightening article (1950).

5 Cf. (1936/37), especially section 7.

6 For a brief survey, see Hempel (1950), section 4.

7 See especially (1936/37), also (1938), Part III.

8 Cf. the decisive analysis in Carnap (1936/37), especially section 15; also see the briefer presentation of the liberalized point of view in Carnap (1938).—The two positions characterized by the definability and the reducibility criterion, respectively, are related also to the points of view which Feigl (1950) discusses under the headings "Naive conventionalistic phenomenalism" and "Critical phenomenalism".

9 Strictly speaking, *no* real number value at all would be permitted by the contemplated rule.

9a Cf. Hilbert (1913).

10 The interpretation of formal theories has been studied extensively by Reichenbach, especially in his pioneer analyses of space and time in classical and in relativistic physics. He describes such interpretation as the establishment of *coordinating definitions* (Zuordnungsdefinitionen) for certain terms of the formal theory. (See, for example, Reichenbach (1928)).—More recently, Northrop (cf. (1947), Ch. VII, and also the detailed study

of the use of deductively formulated theories in science, ibid., Chs. IV, V, VI) and H. Margenau (cf., for example, (1935)) have discussed certain aspects of this process under the title of *"epistemic correlation"*.

11 A somewhat fuller account of this type of interpretation may be found in Carnap (1939), §24.—The articles by Spence (1944) and by MacCorquodale and Meehl (1948) provide enlightening illustrations of the use of theoretical constructs in a field outside that of the physical sciences, and of the difficulties encountered in an attempt to analyze in detail their function and interpretation.

12 What is meant here by explanatory and predictive power in regard to observable phenomena is sufficiently clear for the purposes of the present investigation. A closer study of these concepts, and a proposal for an explicit definition of the concept of systematic, i.e., explanatory and predictive, power of a theoretical system has been presented by Hempel and Oppenheim (1948): Part IV.

13 Cf. Carnap (1936/37), especially sections 8 and 10.

14 Carnap (1936/37), p. 452.

15 The sentence *O* is what Carnap calls the *representative sentence* of the couple consisting of the sentences *S*1 and *S*2; see (1936/37), pp. 450-453.

16 Cf., for example, Carnap $(1945)_1$ and $(1945)_2$, and especially (1950). Also see Helmer and Oppenheim (1945) and, for a critique of these approaches, Reichenbach (1950).

17 On simplicity; cf. especially Popper (1935), Ch. V; Reichenbach (1938), §42; Goodman $(1949)_1$, $(1949)_2$, (1950); on explanatory and predictive power, cf. Hempel and Oppenheim (1948).

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COMMENTS ON PROFESSOR HEMPEL'S "THE CONCEPT OF COGNITIVE SIGNIFICANCE"[1, 2]

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1. INTRODUCTORY

It will be best if I preface these brief comments by two statements of limitation concerning two matters Hempel mentions but which need not be taken up in discussing what is customarily meant by a criterion of meaning. The first of these (a) is more fundamental and, therefore, systematically prior to the problems at hand; the second (b) is technical in a sense in which the core of these problems is not.

(a) Like Hempel, I shall make use of the distinction between *analytic* and *synthetic* sentences and, also, of that between *logical* (structural) and *descriptive* (extralogical) terms. But though I shall not explicate these two dichotomies here, I do not wish to give the impression that I share Hempel's diffidence as to the possibility of establishing "sharp dividing lines between statements of purely logical and statements of empirical significance"; that I do not consider as confusing an extension of the notion of analyticity which makes Carnap's reduction sentences analytic; or that I believe a philosophically significant notion of analyticity could and, perhaps, should be "relativized" in the sense indicated by Hempel. The gist of the matter, as I see it, is this. First, the two distinctions are completely clear and precise in that they can be made within pure syntax. Second, they can be made in a nontrivial manner in the sense in which, say, the setting aside of the logical terms by enumeration would be trivial. To say this is one thing. The familiar difficulties, how to decide whether in certain formal systems (language schemas) a given sentence is in fact analytic, or how we can ever be sure that such a schema is adequate for saying everything we want to say are another thing.

(b) Like Hempel, I believe that the technic known as the *partial interpretation of axiomatic systems* is useful in clarifying the logic or, if you please, the empirical significance of some terms. But I believe this to be true only for a relatively small class of highly specialized terms of theoretical physics such as 'electron' and 'nucleus' [1]. I suggest, therefore, that we fractionate our problem by first looking for a criterion of meaning

that works satisfactorily for a language from which these very special terms have been temporarily expurgated. One reason why this seems wise is that the "coordination" of, say, the "empirical" term 'pressure', defined by means of scales, barometric columns, etc., which would still occur in our expurgated language, to the term 'pressure', as defined in, say, the classical atomic model, can be construed as a purely syntactical affair in exactly the same sense in which a Finnish-Swedish dictionary is for me, who understands neither of these two languages, a matter of syntax. The difference is that in the case at hand the translation rules are very complicated as well as rather limited, and that they are established by explicit agreement for the familiar purposes of scientific explanation. Also, this time we happen to understand one of the two languages; thus we understand, within the limits set by the dictionary, the other.

An objection sometimes raised against this way of construing the particle notions of theoretical physics is that it introduces an invidious distinction between, say, 'chair' and 'electron' and therefore, presumably, between chairs and electrons. Not surprisingly, the intellectual motive behind this objection is, more often than not, some sort of realism, anxious to insist that electrons are as "real" as chairs, if not more so. The proper way of dealing with this objection is, of course, to show, by the usual discursive and indirect method of analytical philosophy, that the proposed technic for construing certain terms does not in fact imply what these objectors fear it does. Clearly, the task is beyond the scope of these comments. It is worth noticing, though, that one step of the clarification consists in showing that even the particle notions of classical physics could, in principle, be introduced by explicit definitions. This, by the way, is also true of all the concepts of scientific psychology [10]. On the other hand, it seems that the philosophical puzzles that have been raised concerning the Schroedinger-Heisenberg theory cannot be dissolved without the technic of partial interpretation of axiomatic systems. With it, I believe I have shown that they disappear [4].

2. The Principle of Acquaintance

What Hempel now calls the *requirement of definability* is, if I understand him correctly, what analytical philosophers know as a *principle of acquaintance* [3]. I neglect, for this occasion, all problems connected with proper names; and I assume, without examining here the warrantedness of this assumption, what

is part of the thesis of Russell's logical atomism, namely, that all descriptive terms of the ideal language are either proper names or predicates, including relational predicates. By 'ideal language' I mean that formally constructed schema in which, when it is once "interpreted", we could, with the exception mentioned and provided for in 1 (b), say "everything" [7, 9]. This being understood, the principle states that all descriptive predicates belong to, or can be explicitly (contextually) defined by means of, a class of terms that "name" characters which are "immediately observable." Or, as I prefer to put it, the principle maintains that a descriptive predicate of the *ideal language* is *either* syntactically primitive (undefined) and in this case belongs to a class C whose members name, upon interpretation, immediately observable characters *or* explicitly definable by means of members of C.

It is well to distinguish questions about the *status* of this principle from those about its *desirability* and, again, from those concerning its *adequacy*, i.e., whether an ideal language can actually be constructed in accordance with it. Presently I shall make a few remarks concerning its status. As to its desirability, I shall only say that the principle is closely related to an idea of philosophical analysis which I, and not I alone, have found very useful, for which I know no substitute, and which I am, therefore, loath to abandon. Very roughly speaking, the idea is that the clarification, or an essential part of the clarification, of the philosophical puzzles raised in connection with a term consists in providing for each sentence which contains it another sentence which does not contain it, so that the statement of equivalence between these two sentences is analytic. The insistence on the exclusive use of explicit definitions serves to secure this *eliminability*, which is no longer guaranteed when one admits other methods, such as Carnap's reduction chains, for "introducing" rather than, literally, defining descriptive predicates. Two supplementary remarks are in order.

(a) Even if, accepting the principle of acquaintance, we restrict ourselves to explicitly defined predicates, we cannot be sure of their eliminability in contexts which are not extensional [6]. But, as I have argued elsewhere [2], there is reason to believe that the ideal language is on the whole extensional. The qualification "on the whole" is necessary because of the nonextensional properties of 'des', a nondescriptive symbol to be interpreted as 'designates' and which the ideal language must contain since it must provide for the clarification of all philosophical puzzles

and because this (or a similar) term is needed to provide for those concerning 'truth' and 'designation' [9].

(b) Even in an extensional schema that admits only explicit definitions, one would, unless a further condition is fulfilled, have to admit into the class C predicates that belong to different types; for instance, in addition to such color words as 'green', 'blue' and 'yellow' also the word 'color'. The condition is that no two undefined descriptive predicates are coextensive [5]. But then, I know of no two characters that are coextensive and at the same time simple enough for their names to be likely candidates for the status of undefined descriptive predicates in the ideal language.

3. The Adequacy of the Principle of Acquaintance

Hempel holds that the principle is inadequate or, as he puts it, "too restrictive." The reason he gives is the alleged necessity of introducing the so-called disposition terms by means of Carnap's reduction chains rather than by explicit definitions. To join the issue once more by means of the old illustration, let 'f_1' and 'f_2' stand for 'put into water' and 'dissolves' respectively. The predicate 'f_3' as explicitly defined by '$f_3(x) \equiv f_1(x) \supset f_2(x)$' is then not regarded as an acceptable analysis or, if you please, reconstruction of 'soluble (in water)'. If this rejection is at all justified, then it is of considerable importance; for (1) it affects, at least, all definitions whose main connective in the definiens is '$\supset$', and (2) a very large number of concepts — all those which, as one sometimes says, are operationally defined — would, if they are to be introduced by explicit definitions from a reasonably limited basis C, be of this form.

I am prepared to maintain that the principle of acquaintance is adequate and, not unnaturally in the light of what has just been said, I shall argue my case by arguing that 'f_3', *which I refrain from transliterating by the English word 'soluble'*, is all we need for an adequate analysis of this English word. Yet I know as well as the critics of explicit definition that 'f_3' is not a synonym of 'soluble', if for no other reason than that it could be truly predicated of a certain match which has been burned before it was ever put into water and of which nevertheless we wish to say that it is (was) not soluble. It must be clear by now that, in my opinion, the key to the problem cannot be

found by more or less formal ingenuity. What is needed is reflection on what we mean by an adequate analysis or, to say the same thing differently, reflection on the proper relation between our ordinary language on the one hand and the ideal language or schema by means of which we analyze it on the other. But perhaps it is well to add here that, as I use the term and as I think it should be used by philosophers, what scientists speak and write is ordinary language.

Consider the English sentence 'If it rains and thunders then it necessarily rains'. What we do in one stage of the analysis of 'necessary', in the connotation it has in this sentence, is to point out that in the ideal language sentences of the form '$p.q \supset p$.' are analytic. What we don't do is look for a "synonym" of 'necessary' in the ideal schema. Again, in explicating the 'because' in the English sentence 'I have a headache because I smoked too much' we do not look for a synonym of 'because' but point out that in the ideal language three sentences of the same form as 'Whenever I smoke too much I have a headache', 'I smoked too much', 'I have a headache' respectively exemplify a syllogistic pattern. In this case, we notice, the analysis requires a whole group of sentences in the ideal schema. Generally, we must be prepared that what we call "translation" into the ideal schema is thus indirect, complex, and dependent on the whole context of this schema.

With this in mind, I propose to analyze the *particular* sentence 'The aforementioned match is (was) not soluble' by means of two sentences of the ideal schema, the first corresponding to 'This match is (was) wooden', the second to the *law* 'No wooden object is soluble'. At this point I shall be told that I have merely pushed the problem back one step since a law (whether or not one of the characters it conjoins is, as in this case, dispositional) cannot be adequately "translated" by a formal implication. In other words, I have run into the problem of counterfactuals which, as is well known and as Hempel again points out, is essentially the same as that of the disposition terms. Here is what I would reply to this criticism. What we mean when we state in ordinary English a law of nature conjoining two characters is indeed not adequately analyzed by '$f_1(x) \supset_x f_2(x)$' *alone,* even if, for the sake of the argument, we assume that 'f_1' and 'f_2' are undefined descriptive predicates and adequate "translations" of the English names of these characters. Such a law is adequately analyzed only by (1) giving a class of sentences of

the ideal language that contains, in addition to '$f_1(x) \supset_x f_2(x)$', pairs of statements, such as '$f_1(a)$', '$f_2(a)$', which correspond to *nontrivial positive* instances of the formal implication; and (2) by pointing out the deductive connections among the sentences of this class. But if this is so, then 'f_3' may well be the second predicate in the formal implication that belongs to the class analyzing the English sentence 'No wooden object is soluble'. For it seems that the familiar deficiencies of 'f_3' will be made up completely by such triples as '$w(b)$', '$f_1(b)$', '$\sim f_2(b)$', with 'w' standing for 'wooden'. However, there are in this case still the *trivial negative* instances to be disposed of, as Hempel has pointed out to me in a private communication. In other words, something must be done about such triples as '$w(c)$', '$\sim f_1(c)$', '$\sim f_2(c)$', all three of which may presumably be true and which yet jointly imply the negation of '$w(x) \supset_x \sim f_3(x)$'. We must therefore add that (3) such triples are excluded from the class of sentences which we consider as an analysis of the law. Similar measures to avoid the snares of material implication will have to be taken for laws conjoining disposition terms. If, in a still more complicated case, 'f_1' and 'f_2' are defined by means of operators, then the analysis is not yet complete; "instances" for the "instances" will have to be added and the procedure repeated as often as is necessary.[3]

I conclude that reduction chains serve no useful purpose, that the principle of acquaintance is adequate, and that the problem of counterfactuals which now causes so much discussion is a pseudoproblem. Nor do I mind saying how surpassingly strange I find the readiness with which Hume's fundamental insight is jettisoned in this discussion by writers who are more or less in the empirical tradition. Historically, this is perhaps a sign that, for better or for worse, a certain type of positivism has run its course.

4. The Status of the Principle of Acquaintance

When a philosopher proposes a criterion of meaning, some other philosopher is likely to ask him: "Can your criterion be applied to itself? Is, in particular, 'immediately observable' a member of your class C or, if not, can it be defined by means of it? If the answer is no, don't you convict yourself of talking nonsense? If the answer is yes, what does it prove? How can you be sure that your criterion is not too narrow? Is it not,

therefore, at best a presupposition which you and your friends introduce dogmatically and sometimes, I fear, even unwittingly while, being such a presupposition, it can and must be argued?" Whoever poses such queries challenges the very idea of a meaning criterion rather than the adequacy of any particular one; to answer them is to clarify what I have called the status of our principle. Again, the right answer, or what I believe to be the right answer, is discursive and indirect. We arrive at it by reflecting on what we do, and why we do it, in philosophical analysis.

Its purpose is the clarification of the philosophical puzzles, those that are historically given to us and those we can think of ourselves. To achieve it, we begin by constructing *formally* that linguistic schema L which I call the ideal language. Then, by *informally discoursing about L in ordinary English* (which, by the way, we also used, for what else could we do, when we formally constructed L), we convince ourselves, as well as we may, of two things. First, if we "interpret" L, then we can, in principle, use it instead of ordinary English to say "everything" we wish to say. Second, we can now unravel the philosophical puzzles. Informal discourse of this kind is, therefore, an essential part of philosophy; it is, at least for me, the part that justifies the whole enterprise.

The principle of acquaintance or, for that matter, any such criterion, belongs to this informal discourse about L. But this does not make it, in any sense I could understand, a premise or presupposition. What happens, rather, is this. I *discover* that I can construct an L which is otherwise satisfactory and which I can understand after I have "interpreted" only those of its descriptive predicates which "name" what is "immediately observable". And, of course, I also discover that this circumstance is of strategic importance in clarifying many traditional puzzles; which, by the way, is the only good reason for calling this sort of thing a criterion or principle. But all this is only part of what needs to be said in reply to the tangle from which I started.

I have throughout put certain expressions, such as 'immediately observable', 'naming', and 'interpreting', between double quotes. This was done to indicate that I used them in informal philosophical discourse. Now one may well ask whether L, which is supposedly comprehensive in the sense that in it we could talk about everything, must not itself contain "translations" of these terms and whether, if it does, this whole idea of philosophical analysis is not vitiated by circularity. Both questions deserve

an answer; also, they are relevant for the problem at hand since every meaning criterion that has ever been proposed contains crucially some of these terms, e.g., 'observable'. Let me then begin with 'observable' or, rather, with 'observing', as the former adds to the latter nothing but that connotation of 'possibility' which can be independently explicated by saying that what is possible is what can be expressed by a synthetic sentence of *L*. As to 'observing', I would say that *L* must indeed contain (descriptive) predicates which allow for a "translation" of the ordinary English verbs for our perceptual and other mental acts [8]. And I also grant that these verbs are by no means unproblematic; for to analyze them by "correctly" translating them into *L* is, in my opinion, the main key to some famous philosophical puzzles that revolve around them [2, 9]. However, and this is the point, in our informal discourse we use these verbs not as they are used in the traditional puzzles but in their ordinary sense, which does not lead to any perplexity; and we find that by thus using them in talking about *L* we can dissolve the perplexities of these puzzles. There is, as far as I can see, nothing vitiatingly circular in this procedure. What goes for 'observing', goes also for such "semantical" words as 'naming' and 'interpreting'. But some may grant the force of these remarks and yet demur against my saying, even though in double quotes, that *L* can speak about "everything". For does it, or could it conceivably say what I just said about it? Thus pressed, I would not mind amending my statement to the effect that by talking about *L* we can clarify everything there is to be clarified, including this talking itself. The very idea of talking about something, for instance, is clarified by the "semantical" part of *L*. This much I would say to such a questioner. Silently I would wonder whether he has really grasped the distinction, which is to me a quite unproblematic matter of common sense, between using a symbol on the one hand and mentioning it on the other.

Historically, this account of the respective roles of formal and informal discourse in philosophical analysis is, it seems to me, only another explication of an old idea. G. E. Moore, I believe, expressed it by his doctrine of common sense; Wittgenstein, in an unfortunately paradoxical and prohibitive manner, by his famous distinction between what can and what cannot be said. And I should also like to think that with his recent distinction between questions internal and questions external to a linguistic framework Carnap[5] approaches again, no matter how hesitatingly, this common ground. But I am not at all sure of this since

he still disparages all external questions and thus also the two all-important ones of the comprehensiveness of the formalism and of its usefulness in clarifying the traditional puzzles, so that we may understand them and do not need to dismiss them by decree.

NOTES

[1] I am indebted to Professor Hempel for letting me use the manuscript of his paper in editing these comments.

[2] Since I shall refer to several of my papers I list them here and cite them in the text by bracketing the numbers under which they appear in this list. 1. "Outline of an empiricist philosophy of physics", *Amer. J. of Physics*, 11, 1943; 2. "A positivistic metaphysics of consciousness", *Mind*, 54, 1945; 3. "Remarks on realism", *Phil. Sci.*, 13, 1946; 4. "The logic of quanta", *Amer. J. of Physics*, 15, 1947; 5. "Conditions for an extensional elementaristic language", *Analysis*, 8, 1948; 6. "Contextual definitions in nonextensional contexts", *J. Symb. Logic*, 13, 1948; 7. "Two criteria for an ideal language", *Phil. Sci.*, 16, 1949; 8. "Professor Ayer's analysis of knowing", *Analysis*, 9, 1949; 9. articles "Logical Positivism" and "Semantics" in *History of Philosophical Systems* (ed. V. Ferm), Philosophical Library, New York, 1950; 10. "The logic of psychological concepts", *Phil. Sci.*, 18, 1951.

[3] In an interesting paragraph of a very recent paper Hempel himself argues substantially the reasonableness of such an analysis through *context:* "Problems and changes in the empiricist criterion of meaning", *Rev. internat. de philosophie*, no. 11, 1950 (p. 59). However, 'context', as philosophers use it, is a very ambiguous term and provides the occasion for another remark that should, perhaps, be made in discussing a meaning criterion.

Some think that even if, say, 'mass' could be analyzed by explicit definitions in some such way as I propose, this would still not exhaust the meaning of 'mass'. The gist of this criticism is that the *laws in which a term occurs* are part of its meaning. E.g., the constancy of mass is, upon this view, a part of the meaning of 'mass' in Newtonian physics. This criticism can be disposed of by distinguishing between two of the many connotations of the English word 'meaning'. The one refers to the way a term is introduced; the other to the laws in which it occurs. I have repeatedly and again in a recent paper [10] insisted on this distinction. Idealistic and pragmatist positions attempt to absorb the first (introduction) of these two meanings of 'meaning' into the second (context); in discussing a "meaning criterion" we are concerned only with the first. See also M. Brodbeck, "The new rationalism: Dewey's theory of induction", *J. of Phil.*, 46, 1949.

[4] 'Descriptive predicate' is here a purely syntactical term. See the remarks in section 1 (a) above.

[5] "Empiricism, semantics, and ontology", *Rev. internat. de philosophie*, no. 11, 1950.

NATURAL AND SCIENTIFIC LANGUAGE

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Natural languages and calculi (constructed scientific languages) differ more basically than is generally assumed by logisticians, and the supposed isomorphy, e.g., between English and the language of *Principia Mathematica,* does not hold.

A natural language — the cultural possession of every known human community — has no limitations for any semantic or pragmatic function; it can express narration, conviction, anger, irony, command, evaluation, truth, falsehood, etc.; it is a *universal* language in the sense that there is nothing which in principle cannot be expressed in it. But just because of these broad social and semiotic implications, the semantic references of a natural language are vague. This is the reason why, when a language is developed for scientific purposes, the special conventions of a calculus are introduced. The transition from a natural language to a scientific calculus, involving the restriction of the full-scale functions of the former to a single one of the latter, is a central problem of philosophy. On the other hand, the mere substitution of mathematical symbols for natural language symbols for reasons of clarity, economy of thought, etc., such as when one transposes "Every human is mortal" into (x) (Human (x) $\rightarrow$ Mortal (x)), or Π Hm—although it might be important for technical reasons or as a matter of convenience—seems to be of a subordinate importance.

Thus, a scientific calculus is always dependent on a natural language; the calculus is always *parasitic.* This fact is overlooked in the treatment of the "meta"-problems, which concentrates solely upon the analysis of the formalized symbolism and disregards the manner in which this symbolism has been derived from a natural language. Also, speakers, writers, and discussants of problems of mathematical calculi always use a natural language with occasional specific symbolism, but not more than would be used by any other specialized group, as e.g., farmers, brokers, or football players. Furthermore, the specific characteristics of scientific calculi cannot be automatically transferred to the domain of natural languages; there is, for instance, no use in speaking of axioms, inference, etc., in a natural language like English, where there are no rules of logical deduction or logical

necessity in the purely lingual usage, and any sequence which pleases the speaker can be used if it conforms with the syntactic rules of English. Nor can the assurance of mathematical logicians that commands, etc., can be expressed within the present framework of a calculus be taken at face value. A transposition of the sentence "Shut up!" can no more be equaled by the proposition "The speaker wishes the addressee to cease speaking" (except for a similar usage of symbolic material) than the sentence "X has a toothache" is equaled by the actual toothache of X. Should a language be constructed, however, which would have the same field of semantic and pragmatic reference as a natural language (as has been done in artificial languages or in pasigraphic systems *à la* Chinese), it would be simply a transposition of a natural language and would imply the same scientific problem as the relation of Esperanto to English and would not pose any new problem for the philosophy of language. Therefore, the contention that a child could learn the language of *Principia Mathematica* instead of English cannot be accepted. How would P. M. nursery rhymes or the emotional outbursts of a child sound?

It would seem necessary to reinvestigate the relation between natural language and the calculi.

PART 3

THE ROLE OF "ABSTRACT OBJECTS" IN SCIENCE

SEMANTICS AND ABSTRACT OBJECTS

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Let me begin by deploring two tendencies in the philosophy of language. One is a tendency to confuse meaning with reference; the other is a tendency to widen excessively the category of singular terms.

The tendency to confuse meaning with reference is the one which Frege took pains to combat, though it was, even in Frege's time, less widespread than the other. There is a long tradition for the distinction between connotation and denotation, or between intension and extension; and this is a distinction squarely between meaning and reference. At the level of singular terms, however, meaning and reference were less clearly distinguished. Perhaps there has been a connection between Mill's doctrine that proper names have no connotation, but only reference, and the popular but mistaken inclination to identify the meaning of a proper name with the object named.

Another type of expression whose meaning has commonly been confused with its reference is the statement. Statements have frequently been treated as names of propositions, these latter being construed as entities of a sort better describable as *meanings* of statements. The confusion in the case of proper names and that in the case of statements are opposite; the name is thought of as meaning the named object, for lack of connotation, whereas the statement is thought of as naming its meaning, for want of anything better to name.

Frege kept meaning and reference sharply separated in all cases. He construed a general term as *naming* its extension — the class of all things of which the term is true — and he construed a statement as naming its truth value. On the other hand, he took the meaning (*Sinn*) of a statement to be the proposition, this being analogous to the intension or idea which is the meaning of a general term. For singular terms, likewise, he recognized meanings, distinct from the objects named.

Actually general terms and statements are themselves singular terms for Frege, since he treats each of these as a name of a single entity — a class or a truth value. So Frege is a prime example of the second tendency I promised to deplore — that

of excessively widening the category of singular terms. But let me postpone this matter until I have said what I have to say about meaning.

When the cleavage between meaning and reference is properly heeded, the problems of what is loosely called semantics become separated into two provinces so fundamentally distinct as not to deserve a joint appellation at all. I shall call these the *theory of meaning* and the *theory of reference*. 'Semantics' would be a good name for the theory of meaning, were it not for the fact that some of the best work in so-called semantics, notably Tarski's, belongs to the theory of reference. The main concepts in the theory of meaning, apart from meaning itself, are *synonymy* (or sameness of meaning), *significance* (or possession of meaning), and *analyticity* (or truth by virtue of meaning). Another is *entailment*, or analyticity of the conditional. The main concepts in the theory of reference are *naming*, *truth* and *denotation* (or 'truth-of', in the sense in which the term 'green' denotes or is true of each green thing). Another is *extension*, or the class of denoted objects; also the notion of *values* of variables. The theory of reference is the locale of the so-called semantical paradoxes, such as Grelling's and Berry's and the Epimenides.

The theory of meaning is not troubled with paradox, but it has troubles of a different order. The most conspicuous question is as to the nature of its objects: what sort of things are meanings? They are evidently intended to be ideas, somehow — mental ideas for some semanticists, Platonic ideas for others. Objects of either sort are so elusive, not to say debatable, that there seems little hope of erecting a significant science about them. It is not even clear, granted meanings, when we have two and when we have one; it is not clear when expressions should be regarded as alike in meaning and when they should not. If a standard of synonymy should be arrived at, we may reasonably expect that the appeal to meanings as entities will not have played a very useful part in the enterprise.

A felt need for meant entities may derive from an earlier failure to appreciate that meaning and naming are distinct. Once the theory of meaning is sharply separated from the theory of reference, it is a short step to recognizing as the business of the theory of meaning simply the synonymy of expressions, the meaningfulness of expressions, and the analyticity or entailment of statements; meanings themselves, as obscure intermediary entities, may well be abandoned.[1] This is a step that Frege did not take.

The notions of synonymy, meaningfulness, analyticity, and entailment are connected in intimate ways. Statements are synonymous, in a broad sense, if their biconditional is analytic, or in other words if they entail each other. Singular terms are synonymous if their identity is analytic. Predicates are synonymous if, when they are applied to variables, their universally quantified biconditional is analytic. An expression is meaningful if synonymous with itself. A statement is analytic if synonymous, in our broad sense, with some arbitrarily chosen specimen — say '$(x)(x = x)$'. But there is great difficulty in tying this well-knit group of concepts down to terms that we really understand. The theory of meaning, even with the elimination of the mysterious meant entities, strikes me as in a comparable state to theology — but with the difference that its notions are blithely used in the supposedly most scientific and hard-headed brands of philosophy.

As an empiricist I consider that the cognitive synonymy of statements should consist in sameness of the empirical conditions of their confirmation. A statement is analytic when its operational condition of verification is, so to speak, the null condition. But I am using terms here which we cannot pretend to understand until we have made substantial progress in the theory of confirmation. Considering what good hands the problem of confirmation is in, it may seem that optimism is in order. However, if synonymy and analyticity and the rest are to be made sense of only in terms of an eventual account of confirmation, I think philosophers are tending to be insufficiently chary of the circularity involved in resting their eventual account of confirmation upon such concepts as synonymy and analyticity.

So much for meaning. Now let me return to the theory of reference, to deplore the practice by Frege and others of excessively widening the category of terms that name. Once the theory of meaning is well separated from that of naming, there is no need to seek a named object in order to insure meaningfulness. I prefer to consider that naming is the function of singular terms in the original narrow sense, excluding general terms and statements. General terms and statements have referential functions of their own, but of a different type than meaning; a general term is *true of* this and that object and *false of* this and that object, and a statement is *true* or *false*. A general term *has* its extension, viz. the class of all things of which it is true, and a statement *has* its truth value; but there is no need to treat the general term as a name of its extension, nor the statement as

a name of its truth value. Names can be formed for classes and for truth values, as for anything else; but I prefer not to regard the general terms and statements as names at all. On this point, though by no means in general, I agree with Carnap's *Meaning and Necessity.*

Why I want to keep general terms and statements out of the category of singular terms will be clearer later. Meanwhile let us consider wherein, in principle, naming consists. I shall assume that we use a language in which quantification is fundamental. Clearly, then, the things which we must recognize as *there being,* in order for our discourse to be true, are just the things which must fall within the range or ranges of our variables of quantification in order that the statements we affirm come out true. This is obvious from the meaning of the quantifiers: 'each object x is such that', 'there is something x such that'. It follows that a term need be regarded as a name of something only if the term is always substitutable for the variable of a universal quantification,[2] without leading from truth to falsehood or nonsense.

To say that Frege treats general terms as names of classes and statements as names of truth values, then, is simply to say that he provides for substitution of general terms for quantified class variables and substitution of statements for quantified truth-value variables.

The disadvantage of this procedure is that it leads to a false accounting of the ontological presuppositions of most of our discourse. When we say that some dogs are black, $(\exists x)$ (x is a dog . x is black), we explicitly admit some black dogs into our universe but do not commit ourselves to such abstract entities as dogkind or the class of black things; hence it is misleading to construe the words 'dog' and 'black' as names of such entities.

Classes are genuinely presupposed in discourse which quantifies over class variables. There are occasions which call quite directly for this sort of discourse; e.g., where we want to say that some zoölogical species are cross-fertile. Another is where we want to define ancestor in terms of parent, by Frege's method: x is ancestor of y if x belongs to *every class* which contains y and all parents of its own members. There is thus serious motive for quantification over classes; and, insofar, there is a place for singular terms which name classes — such singular terms as "dogkind" and "the class of Napoleon's ancestors". But let us not read such terms into positions where no appeal to classes is involved.

I would distinguish, therefore, between:

$$(\exists x)\ (x \text{ is-a-dog} \ .\ x \text{ is-black}) \quad \text{and} \quad (1)$$

$$(\exists x)\ (x \in \text{dogkind} \ .\ x \in \text{class-of-black-things}). \quad (2)$$

(2) is a case of the form:

$$(\exists x)\ (x \in y \ .\ x \in z), \quad \text{or:} \quad (\exists x)\ (x \in \alpha \ .\ x \in \beta).$$

The "dogkind" and "class-of-black-things" in (2) are singular terms, capable of occurring in positions appropriate to variables of quantification. On the other hand the general terms or predicates "is-a-dog" and "is-black", in (1), are not to be regarded as meaningfully substitutable for variables. I would represent the form of (1) merely by a *schema* '$(\exists x)\ (Fx \ .\ Gx)$', in which the '$F$' and '$G$' are purely diagrammatic and do not admit of quantification.

I grant that (1) and (2) as wholes are equivalent statements. But they differ in that (1) belongs to the part of language which is neutral on the question of class existence, whereas (2) is a degenerate specimen of the higher part of language. It is degenerate merely in that it happens as a whole to be equivalent to (1).

The bulk of logical reasoning takes place on a level which does not presuppose abstract entities. Such reasoning proceeds mostly by quantification theory, the laws of which can be represented through schemata involving no quantification over class variables. Much of what is commonly formulated in terms of the algebra of classes or the algebra of relations actually needs no such things as classes and relations, and can be easily reformulated schematically within quantification theory plus perhaps identity theory.

Thus I consider it a defect in an all-purpose formulation of the theory of reference if, like Frege's, the formulation represents us as referring to abstract entities from the very beginning rather than only where there is a real purpose in such reference. Hence my wish to keep general terms distinct from abstract singular terms.

I am not here arguing over-all repudiation of abstract entities. In discussing the theory of meaning I did urge the uselessness of meanings as entities. My thesis there was, in effect, that no gain is to be sought in quantifying over such alleged entities as meanings. But quantification over classes or other abstract entities is the essence of set theory and indeed of most

of classical mathematics; and mathematics is, figuratively as well as literally, something to reckon with. I urge only that we put no obstacles in the way of distinguishing clearly between that part of discourse which depends squarely on an ontology of abstract entities and that part which does not.

In depriving general terms or predicates of the virtues of names I do not thereby deny that there are certain classes connected with them otherwise than in the fashion of being named. On the contrary, in that part of the theory of reference which has to do with classes there is occasion to speak of the *extension of* a general term or predicate — the class of all things of which the predicate is true. There is occasion in that part of the theory of reference to assume such classes and to quantify over them. One such occasion arises when in the theory of reference we treat the topic of validity of schemata of pure quantification theory; for a quantificational schema is valid when it comes out true for all assignments of classes as extensions of the schematic predicate letters. The general theory of quantificational validity thus appeals to classes, but the individual statements represented by the schemata of quantification theory need not; the statement '$(\exists x)$ (x is a dog . x is black)' involves, of itself, no appeal to the abstract extension of a predicate.

Similarly there is occasion in the theory of validity to speak of truth values of statements, e.g., in defining truth-functional validity. But there is no need to treat statements as names of these values, nor as names at all. When we simply affirm a statement we do not thereby appeal to any such entity as truth values, unless the statement happens to have that special subject matter.

It can indeed prove convenient and elegant in special systems to reconstrue statements as names — perhaps of 0 and 1, as in Church's system. This is perhaps better regarded as a matter of making names of 0 and 1 serve the purpose of statements, for the special system; and I have no quarrel with it. Similarly Frege may be represented as making his singular terms, plus membership, do the work of general terms; and with this again, as a means merely of absorbing lower logic into a particular system of higher logic for the sake of elegance, there is no quarrel. What I object to is projection of these special expedients into the general theory of reference. It is preferable in the general theory of reference to proceed in such a way as not to impute special ontological presuppositions to portions of discourse which are innocent of them.

Even in the higher theory of validity it happens that the appeal to truth values of statements and extensions of predicates can finally be eliminated. For, truth-functional validity can be redefined by the tabular test of computation, and validity in quantification theory can be redefined simply by appeal to the rules of proof (since Gödel has proved them complete). Here is a good example of the elimination of ontological presuppositions, in one particular domain.

In general it is important, I think, to show how the purposes of a certain segment of mathematics can be met with a reduced ontology, just as it is important to show how an erstwhile nonconstructive proof in mathematics can be accomplished by constructive means. The interest in progress of this type is no more dependent upon an out-and-out intolerance of abstract entities than it is upon an out-and-out intolerance of nonconstructive proof. The important thing is to understand our instrument; to keep tab on the diverse presuppositions of diverse portions of our theory, and reduce them where we can. It is thus that we shall best be prepared to discover, eventually, the over-all dispensability of some assumption that has always rankled as *ad hoc* and unintuitive.

[1] When I presented this paper at the meeting of the Institute for the Unity of Science in April, 1950, I had no thought of publication. Eight months later, at the Toronto meeting of the American Philosophical Association, I presented my paper "Two dogmas of empiricism," and it appeared in the *Philosophical Review* in January, 1951. I now observe in reading proof that the above nineteen lines recurred almost verbatim in "Two dogmas," pp. 22 f, though in the main the two papers are unlike in content.

[2] With appropriate type of variable, in case there are distinctions.

COMMENTS ON PRECEDING PAPER OF W. V. QUINE

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"When the cleavage between meaning and reference is properly heeded," says Professor Quine, "the problems of what is loosely called semantics become separated into two provinces so fundamentally distinct as not to deserve a joint appellation at all." If this is right, it would be helpful to be told on what principle the separation is to be made. What, if anything, do questions about reference have in common?

Frege's doctrines will not help us in answering this question. Indeed it seems to me a mistake to try to connect Quine's partition of semantics with the distinction between *Bedeutung* and *Sinn*. For one thing, each of these everyday German words would be translated by a dictionary as equivalent to "meaning," and Frege himself thought of them as referring to different aspects of meaning; his work would have been hamstrung by any prohibition against discussing the relations between them in one and the same context. Would his paper on "*Sinn und Bedeutung*" need to be relegated to yet a third "province" if Quine's admonition was heeded?

It would have annoyed Frege to be told that "he construed a general term as *naming* its extension—the class of all things of which the term is true." Time and again Frege made it plain that a grammatical predicate stood for a *Begriff* (concept), that is to say for one of a special kind of first-level *function* having only truth-values as its values. In order to name the extension of a concept he used not a general term but a special complex sign (roughly equivalent to a class-name in *Principia Mathematica*). He made the sharpest of distinctions between an object (*Gegenstand*) and a function; it is therefore misleading, to say the least, to claim that Frege took a general term to be "a name of a single entity." And for Frege the relation between a concept and the things "falling under" that concept, as he always said, was emphatically not the relation of *Bedeutung*.

The question before us, however, is not what Frege meant by "*Bedeutung*" but rather what Quine means by "reference." "The main concepts in the theory of reference," he says, "are *naming*, *truth*, and *denotation* (or 'truth-of,' in the sense in

which the term 'green' denotes or is true of each green thing). Another is *extension,* or the class of denoted objects; also the notion of *values* of variables." Is the idea behind this list that all of these notions are used to say what is the case, rather than what words and signs *mean*? No doubt the semanticist (unfortunate word) need not bother whether 'Ossian' was any real person's name any more than he need worry about the truth of the *Iliad* or wonder what song the Syrens sang. But he is not expected to answer specific questions about meaning either. If his job is to *analyze* the meanings of "synonymy," "significance," and so forth, it is not clear that he can dispense with talk about truth-conditions, i.e., about what *would* be the case. When Quine says that "the cognitive synonymy of statements should consist in sameness of the empirical conditions of their confirmation" he comes close to saying as much himself.

However, there is little profit in haggling about a proposal to divide a field of study into separate provinces. The proof of the semantics is the axiomatization thereof, and when Quine has provided an analytical account of synonymy and related notions making no use of notions belonging to the theory of reference I shall be the first to cheer. It is good that the attempt should be made. On the other hand, I cannot see anything that needs to be "deplored" in "the practice of Frege and others" of using names and descriptions to stand for meanings. It would be naive to suppose that an expression like "the meaning of *S*" was in no need of logical anchorage to clearer idioms. But as much could be said of any technical expression in semantics; I cannot see that "naming" is especially vulnerable to criticism.

As to the alleged "ontological presuppositions" of such discourse, I wish it were possible to use a more aseptic description of what Quine has in mind. The formula, "To be is to be the value of a variable"[1] is too attractively paradoxical not to engender confusion. Like other paradoxes, its charm depends upon its patent falsity. Anybody who takes it at face value by substituting for the second occurrence of the words "to be" in the formula itself will derive the Stein-ish formula "To be is to be the value of a variable the value of a variable." And if the formula is applied to such a sentence as "There are wild Pandas," the result is the sentence "Wild Pandas are values of variables." This is hardly what can be meant by saying there are wild Pandas. A variable is a certain sign, like 'x', having a distinctive and familiar set of uses in logical and mathematical notation; it would seem, then, that the existence of Pandas de-

pends upon the existence of signs like 'x' and 'y'. Could it be that there were no wild Pandas before logicians began using letters from the end of the alphabet as place-holders?

If we take the reference to variables less strictly, we get something like the following interpretation: To find out whether somebody "countenances" wild Pandas (to use another charming idiom), find out whether he is prepared to use a pronoun in place of "a wild Panda" or, better still, whether he is prepared to substitute "something." A bedtime story might well begin: "A phoenix approached the bed where John lay sleeping. *He* growled softly, so John knew *something* was in the room." Must the storyteller believe in phoenixes? Is the mathematician who talks about parallel lines intersecting at a point at infinity "ontologically committed" to there being a point at infinity? Surely it is impossible to settle such questions by any formal criterion that relies upon the use of expressions like 'there is' or 'he' or 'something.' There is no formal test to determine the difference between make-believe and serious assertion, or between a mere manner of speech and an "ontological commitment"—whatever that may be.

I cannot see that anything that has traditionally been called "ontology" is involved in the technical question of how economical one can be in using quantification in a rigorous formal development of semantics. The technical question has considerable interest in its own right and has everything to gain from being set free from its supposed philosophical implications.

[1] See Quine's article, "Designation and Existence," *Journal of Philosophy*, 1939. Cf. "To be is, purely and simply, to be the value of a variable" (*Review of Metaphysics*, 1948, p. 32).

THE NEED FOR ABSTRACT ENTITIES IN SEMANTIC ANALYSIS

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We distinguish between a *logistic system* and a *formalized language* on the basis that the former is an abstractly formulated calculus for which no interpretation is fixed, and thus has a syntax but no semantics; but the latter is a logistic system together with an assignment of meanings to its expressions.

As primitive basis of a logistic system it suffices to give, in familiar fashion: (1) The list of primitive symbols or *vocabulary* of the system (together usually with a classification of the primitive symbols into categories, which will be used in stating the formation rules and rules of inference). (2) The *formation rules*, determining which finite sequences of primitive symbols are to be *well-formed* expressions, determining certain categories of well-formed expressions — among which we shall assume that at least the category of *sentence* is included—, and determining (in case *variables* are included among the primitive symbols) which occurrences of variables in a well-formed expression are *free* occurrences and which are *bound* occurrences.[1] (3) The transformation rules or *rules of inference*, by which from the *assertion* of certain sentences (the *premisses*, finite in number) a certain sentence (the *conclusion*) may be *inferred*. (4) Certain asserted sentences, the *axioms*.

In order to obtain a formalized language it is necessary to add, to these *syntactical rules* of the logistic system, *semantical rules* assigning meanings (in some sense) to the well-formed expressions of the system.[2] The character of the semantical rules will depend on the theory of meaning adopted, and this in turn must be justified by the purpose which it is to serve.

Let us take it as our purpose to provide an abstract theory of the actual use of language for human communication — not a factual or historical report of what has been observed to take place, but a norm to which we may regard every-day linguistic behavior as an imprecise approximation, in the same way that e.g. elementary (applied) geometry is a norm to which we may regard as imprecise approximations the practical activity of the land-surveyor in laying out a plot of ground, or of the construc-

tion foreman in seeing that building plans are followed. We must demand of such a theory that it have a place for all observably informative kinds of communication — including such notoriously troublesome cases as belief statements, modal statements, conditions contrary to fact — or at least that it provide a (theoretically) workable substitute for them. And solutions must be available for puzzles about meaning which may arise, such as the so-called "paradox of analysis."

There exist more than one theory of meaning showing some promise of fulfilling these requirements, at least so far as the formulation and development have presently been carried. But the theory of Frege seems to recommend itself above others for its relative simplicity, naturalness, and explanatory power —or, as I would advocate, Frege's theory as modified by elimination of his somewhat problematical notion of a function (and in particular of a *Begriff*) as *ungesättigt*, and by some other changes which bring it closer to present logistic practice without loss of such essentials as the distinction of sense and denotation.

This modified Fregean theory may be roughly characterized by the tendency to minimize the category of *syncategorematic* notations — i.e., notations to which no meaning at all is ascribed in isolation but which may combine with one or more meaningful expressions to form a meaningful expression[3] — and to reduce the categories of meaningful expressions to two, (proper) *names* and *forms*, for each of which two kinds of meaning are distinguished in a parallel way.

A name, or a *constant* (as we shall also say, imitating mathematical terminology), has first its *denotation*, or that of which it is a name.[4] And each name has also a *sense* — which is perhaps more properly to be called its meaning, since it is held that complete understanding of a language involves the ability to recognize the sense of any name in the language, but does not demand any knowledge beyond this of the denotations of names. (Declarative) *sentences*, in particular, are taken as a kind of names, the denotation being the *truth-value* of the sentence, *truth* or *falsehood*, and the sense being the *proposition* which the sentence expresses.

A name is said to *denote* its denotation and to *express* its sense, and the sense is said to be *a concept of* the denotation. The abstract entities which serve as senses of names let us call *concepts* — although this use of the word 'concept' has no analogue in the writings of Frege, and must be carefully distinguished from Frege's use of 'Begriff.' Thus anything which is

or is capable of being the sense of some name in some language, actual or possible, is a concept.[5] The terms *individual concept*, *function concept*, and the like are then to mean a concept which is a concept of an individual, of a function, etc. A *class concept* may be identified with a *property*, and a *truth-value concept* (as already indicated) with a proposition.

Names are to be meaningful expressions without free variables, and expressions which are analogous to names except that they contain free variables, we call forms (a rather wide extension of the ordinary mathematical usage, here adopted for lack of a better term).[6] Each variable has a *range*, which is the class of admissible *values* of the variable.[7] And analogous to the denotation of a name, a form has a *value* for every system of admissible values of its free variables.[8]

The assignment of a value to a variable, though it is not a syntactical operation, corresponds in a certain way to the syntactical operation of substituting a constant for the variable. The denotation of the substituted constant represents the value of the variable.[9] And the sense of the substituted constant may be taken as representing a *sense-value* of the variable. Thus every variable has, besides its range, also a *sense-range*, which is the class of admissible sense-values of the variable. And analogous to the sense of a name, a form has a *sense-value* for every system of admissible sense-values of its free variables.[10]

The following principles are assumed:[11] (i) Every concept is a concept of at most one thing. (ii) Every constant has a unique concept as its sense. (iii) Every variable has a non-empty class of concepts as its sense-range. (iv) For any assignment of sense-values, one to each of the free variables of a given form, if each sense-value is admissible in the sense that it belongs to the sense-range of the corresponding variable, the form has a unique concept as its sense-value. (v) The denotation of a constant is that of which its sense is a concept. (vi) The range of a variable is the class of those things of which the members of the sense-range are concepts. (vii) If S, s_1, s_2, ..., s_m are concepts of A, a_1, a_2, ..., a_m respectively, and if S is the sense-value of a form F for the system of sense-values s_1, s_2, ..., s_m of its free variables x_1, x_2, ..., x_m, then the value of F for the system of values a_1, a_2, ... a_m of x_1, x_2, ..., x_m is A. (viii) If C′ is obtained from a constant C by replacing a particular occurrence of a constant c by a constant c′ that has the same

sense as c, then C′ is a constant having the same sense as C.[12] (ix) If C′ is obtained from a constant C by replacing a particular occurrence of a constant c by a constant c′ that has the same denotation as c, then C′ is a constant having the same denotation as C.[13] (x) If C′ is obtained from a constant C by replacing a particular occurrence of a form f by a form f′ that has the same free variables as f, and if, for every admissible system of sense-values of their free variables, f and f′ have the same sense-value, then C′ is a constant having the same sense as C.[12] (xi) If C′ is obtained from a constant C by replacing a particular occurrence of a form f by a form f′ that has the same free variables as f, and if, for every system of values of their free variables which are admissible in the sense that each value belongs to the range of the corresponding variable, f and f′ have the same value, then C′ is a constant having the same denotation as C.[13] (xii) If $x_1, x_2, \ldots, x_m$ are all the distinct variables occurring (necessarily as bound variables) in a constant C, if $y_1, y_2, \ldots, y_m$ are distinct variables having the same sense-ranges as $x_1, x_2, \ldots, x_m$ respectively, and if C′ is obtained from C by substituting $y_1, y_2, \ldots, y_m$ throughout for $x_1, x_2, \ldots, x_m$ respectively, then C′ is a constant having the same sense as C. (xiii) If $x_1, x_2, \ldots, x_m$ are the distinct variables occurring in a constant C, if $y_1, y_2, \ldots, y_m$ are distinct variables having the same ranges as $x_1, x_2, \ldots, x_m$ respectively, and if C′ is obtained from C by substituting $y_1, y_2, \ldots, y_m$ throughout for $x_1, x_2, \ldots, x_m$ respectively, then C′ is a constant having the same denotation as C. (xiv) The result of substituting constants for all the free variables of a form is a constant, if the sense of each substituted constant belongs to the sense-range of the corresponding variable.[12] (xv) The sense of a constant C thus obtained by substituting constants $c_1, c_2, \ldots, c_m$ for the free variables $x_1, x_2, \ldots, x_m$ of a form F is the same as the sense-value of F when the senses of $c_1, c_2, \ldots, c_m$ are assigned as the sense-values of $x_1, x_2, \ldots, x_m$.

To these must still be added principles which are similar to (viii)–(xv), except that substitution is made in forms instead of constants, or that forms and variables as well as constants are substituted for the free variables of a form. Instead of stating these here, it may be sufficient to remark that they follow

if arbitrary extensions of the language are allowed by adjoining (as primitive symbols) constants which have as their senses any concepts that belong to sense-ranges of variables in the language, if the foregoing principles are assumed to hold also for such extensions of the language, and if there is assumed further:
(xvi) Let an expression F contain the variables $x_1, x_2, \ldots, x_m$; and suppose that in every extension of the language of the kind just described and for every substitution of constants $c_1, c_2, \ldots, c_m$ for the variables $x_1, x_2, \ldots, x_m$ respectively, if the sense of each constant belongs to the sense-range of the corresponding variable, F becomes a constant; then F is a form having $x_1, x_2, \ldots, x_m$ as its free variables.

To those who find forbidding the array of abstract entities and principles concerning them which is here proposed, I would say that the problems which give rise to the proposal are difficult and a simpler theory is not known to be possible.[14]

To those who object to the introduction of abstract entities at all I would say that I believe that there are more important criteria by which a theory should be judged. The extreme demand for a simple prohibition of abstract entities under all circumstances perhaps arises from a desire to maintain the connection between theory and observation. But the preference of (say) *seeing* over *understanding* as a method of observation seems to me capricious. For just as an opaque body may be seen, so a concept may be understood or grasped. And the parallel between the two cases is indeed rather close. In both cases the observation is not direct but through intermediaries — light, lens of eye or optical instrument, and retina in the case of the visible body, linguistic expressions in the case of the concept. And in both cases there are or may be tenable theories according to which the entity in question, opaque body or concept, is not assumed, but only those things which would otherwise be called its effects.

The variety of entities (whether abstract or concrete) which a theory assumes is indeed one among other criteria by which it may be judged. If multiplication of entities is found beyond the needs of the workability, simplicity, and generality of the theory, then the razor shall be applied.[15] The theory of meaning here outlined I hold exempt from such treatment no more than any other, but I do advocate its study.

Let us return now to our initial question, as to the character of the semantical rules which are to be added to the syntactical rules of a logistic system in order to define a particular formalized language.

On the foregoing theory of meaning the semantical rules must include at least the following: (5) *Rules of sense,* by which a sense is determined for each well-formed expression without free variables (all such expressions thus becoming names). (6) *Rules of sense-range,* assigning to each variable a sense-range. (7) *Rules of sense-value,* by which a sense-value is determined for every well-formed expression containing free variables and every admissible system of sense-values of its free variables (all such expressions thus becoming forms).

In the case of both syntactical and semantical rules there is a distinction to be drawn between *primitive* and *derived* rules, the primitive rules being those which are stated in giving the primitive basis of the formalized language, and the derived rules being rules of similar kind which follow as consequences of the primitive rules. Thus besides primitive rules of inference there are also derived rules of inference, besides primitive rules of sense also derived rules of sense, and so on. (But instead of "derived axioms" it is usual to say *theorems.*)

A statement of the denotation of a name, the range of a variable, or the value of a form does not necessarily belong to the semantics of a language. For example, that 'the number of planets' denotes the number nine is a fact as much of astronomy as it is of the semantics of the English language, and can be described only as belonging to a discipline broad enough to include both semantics and astronomy. On the other hand, a statement that 'the number of planets' denotes the number of planets is a purely semantical statement about the English language. And indeed it would seem that a statement of this kind may be considered as purely semantical only if it is a consequence of the rules of sense, sense-range, and sense-value, together with the syntactical rules and the general principles of meaning (i)–(xvi).

Thus as derived semantical rules rather than primitive, there will be also: (8) *Rules of denotation,* by which a denotation is determined for each name. (9) *Rules of range,* assigning to each variable a range. (10) *Rules of value,* by which a value is determined for every form and for every admissible system of values of its free variables.

By stating (8), (9), and (10) as primitive rules, without (5),

(6), and (7) there results what may be called the *extensional part* of the semantics of a language. The remaining *intensional part* of the semantics does not follow from the extensional part. For the sense of a name is not uniquely determined by its denotation, and thus a particular rule of denotation does not of itself have as a consequence the corresponding rule of sense.

On the other hand, because the meta-linguistic phrase which is used in the rule of denotation must itself have a sense, there is a certain sense (though not that of logical consequence) in which the rule of denotation, by being given as a primitive rule of denotation, uniquely indicates the corresponding rule of sense. Since the like is true of the rules of range and rules of value, it is permissible to say that we have fixed an *interpretation* of a given logistic system, and thus a formalized language, if we have stated only the extensional part of the semantics.[16]

Although all the foregoing account has been concerned with the case of a formalized language, I would go on to say that in my opinion there is no difference in principle between this case and that of one of the natural languages. In particular, it must not be thought that a formalized language depends for its meaning or its justification (in any sense in which a natural language does not) upon some prior natural language, say English, through some system of translation of its sentences into English — or, more plausibly, through the statement of its syntactical and semantical rules in English. For speaking in principle, and leaving all questions of practicality aside, the logician must declare it a mere historical accident that you and I learned from birth to speak English rather than a language with less irregular, and logically simpler, syntactical rules, similar to those of one of the familiar logistic systems in use today — or that we learned in school the content of conventional English grammars and dictionaries rather than a more precise statement of a system of syntactical and semantical rules of the kind which has been described in this present sketch. The difference of a formalized language from a natural language lies not in any matter of principle, but in the degree of completeness that has been attained in the laying down of explicit syntactical and semantical rules and the extent to which vaguenesses and uncertainties have been removed from them.

For this reason the English language itself may be used as a convenient though makeshift illustration of a language for which

syntactical and semantical rules are to be given. Of course only a few illustrative examples of such rules can be given in brief space. And even for this it is necessary to avoid carefully the use of examples involving English constructions that raise special difficulties or show too great logical irregularities, and to evade the manifold equivocacy of English words by selecting and giving attention to just one meaning of each word mentioned. It must also not be asked whether the rules given as examples are among the "true" rules of the English language or are "really" a part of what is implied in an understanding of English; for the laying down of rules for a natural language, because of the need to fill gaps and to decide doubtful points, is as much a process of legislation as of reporting.

With these understandings, and with no attempt made to distinguish between primitive and derived rules, following are some examples of syntactical and semantical rules of English according to the program which has been outlined.[17]

(1) Vocabulary: 'equals' 'five' 'four' 'if' 'is' 'nine' 'number' 'of' 'planet' 'planets' 'plus' 'round' 'the' 'then' 'the world' — besides the bare list of primitive symbols (words) there must be statements regarding their classification into categories and systematic relations among them, e.g., that 'planet' is a common noun,[18] that 'planets' is the plural of 'planet,'[19] that 'the world' is a proper noun, that 'round' is an adjective.

(2) Formation Rules: If A is the plural of a common noun, then 'the'⌢'number'⌢'of'⌢A is a singular term. A proper noun standing alone is a singular term. If A and B are singular terms, then A⌢'equals'⌢B is a sentence. If A is a singular term and B is an adjective, then A⌢'is'⌢B is a sentence.[20] If A and B are sentences, then 'if'⌢A⌢'then'⌢B is a sentence. — Here singular terms and sentences are to be understood as categories of well-formed expressions; a more complete list of formation rules would no doubt introduce many more such.

(3) Rules of Inference: Where A and B are sentences, from 'if'⌢A⌢'then'⌢B and A to infer B. Where A and B are singular terms and C is an adjective, from A⌢'equals'⌢B and B⌢'is'⌢C to infer A⌢'is'⌢C.

(4) Axioms-Theorems: 'if the world is round, then the world is round'; 'four plus five equals nine.'

(5) Rules of Sense: 'round' expresses the property of roundness. 'the world' expresses the (individual) concept of the world. 'the world is round' expresses the proposition that the world is round.

(8) Rules of Denotation: 'round' denotes the class of round things. 'the world' denotes the world. 'the world is round' denotes the truth-value thereof that the world is round.[21]

On a Fregean theory of meaning, rules of truth in Tarski's form — e.g., " 'the world is round' is true if and only if the world is round" — follow from the rules of denotation for sentences. For that a sentence is true is taken to be the same as that it denotes truth.

NOTES

1 For convenience of the present brief exposition we make the simplifying assumption that sentences are without free variables, and that only sentences are asserted.

2 The possibility that the meaningful expressions may be a proper subclass of the well-formed expressions must not ultimately be excluded. But again for the present sketch it will be convenient to treat the two classes as identical—the simplest and most usual case. Compare, however, footnote 13.

3 Such notations can be reduced to at most two, namely the notation (consisting, say, of juxtaposition between parentheses) which is used in application of a singulary function to its argument, and the abstraction operator λ. By the methods of the Schönfinkel-Curry combinatory logic it may even be possible further to eliminate the abstraction operator, and along with it the use of variables altogether. But this final reduction is not contemplated here—nor even necessarily the simpler reduction to two syncategorematic notations.

4 The complicating possibility is here ignored of *denotationless names*, or names which have a sense but no denotation. For though it may be held that these do occur in the natural languages, it is possible, as Frege showed, to construct a formalized language in such a way as to avoid them.

5 This is meant only as a preliminary rough description. In logical order, the notion of a concept must be postulated and that of a possible language defined by means of it.

6 Frege's term in German is *Marke*.—The form or *Marke* must of course not be confused with its associated abstract entity, the *function*. The function differs from the form in that it is not a linguistic entity, and belongs to no particular language. Indeed the same function may be associated with different forms; and if there is more than one free variable the same form may have several associated functions. But in some languages it is possible from the form to construct a name (or names) of the associated function (or functions) by means of an abstraction operator.

7 The idea of allowing variables of different ranges is not Fregean, except in the case of functions in Frege's sense (i.e., as *ungesättigt*), the different categories of which appear as ranges for different variables. The intro-

duction of *Gegenstandsbuchstaben* with restricted ranges is one of the modifications here advocated in Frege's theory.

8 Exceptions to this are familiar in common mathematical notation. E.g. the form x/y has no value for the system of values 0,0 of x, y. However, the semantics of a language is much simplified if a value is assigned to a form for every system of values of the free variables which are admissible in the sense that each value belongs to the range of the corresponding variable. And for purposes of the present exposition we assume that this has been done. (Compare footnote 4.)

9 Even if the language contains no constant denoting the value in question, it is possible to consider an extension of the language obtained by adjoining such a constant.

10 The notion of a sense-value of a form is not introduced by Frege, at least not explicitly, but it can be argued that it is necessarily implicit in his theory. For Frege's question, "How can a=b if true ever differ in meaning from a=a?" can be asked as well for forms a and b as for constants, and leads to the distinction of value and sense-value of a form just as it does to the distinction of denotation and sense of a constant. Even in a language like that of *Principia Mathematica*, having no forms other than propositional forms, a parallel argument can be used to show that from the equivalence of two propositional forms A and B the identity in meaning of A and B in all respects is not to be inferred. For otherwise how could A $\equiv$ B if true (i.e., true for all values of the variables) ever differ in meaning from A $\equiv$ A?

11 For purposes of the preliminary sketch, the meta-language is left unformalized, and such questions are ignored as whether the meta-language shall conform to the theory of types or to some alternative such as transfinite type theory or axiomatic set theory. Because of the extreme generality which is attempted in laying down these principles, it is clear that there may be some difficulty in rendering them precise (in their full attempted generality) by restatement in a formalized meta-language. But it should be possible to state the semantical rules of a particular object language so as to conform, so that the principles are clarified to this extent by illustration.

It is not meant that the list of principles is necessarily complete or in final form, but rather a tentative list is here proposed for study and possible amendment. Moreover it is not meant that it may not be possible to formulate a language not conforming to the principles, but only that a satisfactory general theory may result by making conformity to these principles a part of the definition of a formalized language (compare footnote 12).

12 In the case of some logistic systems which have been proposed (e.g., by Hilbert and Bernays), if semantical rules are to be added, in conformity with the theory here described and with the informally intended interpretation of the system, it is found to be impossible to satisfy (viii), (x), and (xiv), because of restriction imposed on the bound variables which may appear in a constant or form used in a particular context. But it would

seem that modifications in the logistic system necessary to remove the restriction may reasonably be considered non-essential, and that in this sense (viii), (x), (xiv) may still be maintained.

In regard to all of the principles it should be understood that non-essential modifications in existing logistic systems may be required to make them conform. In particular the principles have been formulated in a way which does not contemplate the distinction in typographical style between free and bound variables that appears in systems of Frege and of Hilbert-Bernays.

In (x) and (xi), the condition that f′ have the same free variables as f can in many cases be weakened to the condition that every free variable of f′ occur also as a free variable of f.

[13] Possibly (ix) and (xi) should be weakened to require only that if C′ is well-formed then it is a constant having the same denotation as C. Since there is in general no syntactical criterion by which to ascertain whether two constants c and c′ have the same denotation, or whether two forms have always the same values, there is the possibility that the stronger forms of (ix) and (xi) might lead to difficulty in some cases. However, (ix) as here stated has the effect of preserving fully the rule of substitutivity of equality—where the equality sign is so interpreted that $[c_1 = c_2]$ is a sentence denoting truth if and only if c_1 and c_2 are constants having the same denotation—and if in some formalized languages, (ix) and (xi) should prove to be inconsistent with the requirement that every well-formed expression be meaningful (footnote 2), it may be preferable to abandon the latter. Indeed the preservation of the rule of substitutivity of equality may be regarded as an important advantage of a Fregean theory of meaning over some of the alternatives that suggest themselves.

[14] At the present stage it cannot be said with assurance that a modification of Frege's theory will ultimately prove to be the best or the simplest. Alternative theories demanding study are: the theory of Russell, which relies on the elimination of names by contextual definition to an extent sufficient to render the distinction of sense and denotation unnecessary; the modification of Russell's theory, briefly suggested by Smullyan (in *The Journal of Symbolic Logic*, vol. 13 (1948), pp. 31-37), according to which descriptive phrases are to be considered as actually contained in the logistic system rather than being (in the phrase of Whitehead and Russell) "mere typographical conveniences," but are to differ from names in that they retain their need for scope indicators; and finally, the theory of Carnap's *Meaning and Necessity*.

Though the Russell theory has an element of simplicity in avoiding the distinction of two kinds of meaning, it leads to complications of its own of a different sort, in connection with the matter of scope of descriptions. The same should be said of Smullyan's proposed modification of the theory. And the distinctions of scope become especially important in modal statements, where they cannot be eliminated by the convention of always taking the minimum scope, as Smullyan has shown (loc. cit.).

Moreover, in its present form it would seem that the Russell theory

requires some supplementation. For example, 'I am thinking of Pegasus,' 'Ponce de Leon searched for the fountain of youth,' 'Barbara Villiers was less chaste than Diana' cannot be analyzed as '$(Ec)[x$ is a Pegasus $\equiv_x x=c]$ [I am thinking of c],' '$(Ec)[x$ is a fountain of youth $\equiv_x x=c]$ [Ponce de Leon searched for c],' '$(Ec)[x$ is a Diana $\equiv_x x=c]$ [Barbara Villiers was less chaste than c]' respectively—if only because of the (probable or possible) difference of truth-value between the given statements and their proposed analyses. On a Fregean theory of meaning the given statements might be analyzed as being about the individual concepts of Pegasus, of the fountain of youth, and of Diana rather than about some certain winged horse, some certain fountain, and some certain goddess. For the Russell theory it might be suggested to analyze them as being about the property of being a Pegasus, the property of being a fountain of youth, and the property of being a Diana. This analysis in terms of properties would also be possible on a Fregean theory, though perhaps slightly less natural. On a theory of the Russell type the difficulty arises that names of properties seem to be required, and on pain of readmitting Frege's puzzle about equality (which leads to the distinction of sense and denotation in connection with names of any kind), such names of properties either must be analyzed away by contextual definition—it is not clear how—or must be so severely restricted that two names of the same property cannot occur unless trivially synonymous.

[15] Here a warning is necessary against spurious economies, since not every subtraction from the entities which a theory assumes is a reduction in the variety of entities.

For example, in the simple theory of types it is well known that the individuals may be dispensed with if classes and relations of all types are retained; or one may abandon also classes and relations of the lowest type, retaining only those of higher type. In fact any finite number of levels at the bottom of the hierarchy of types may be deleted. But this is no reduction in the variety of entities, because the truncated hierarchy of types, by appropriate deletions of entities in each type, can be made isomorphic to the original hierarchy—and indeed the continued adequacy of the truncated hierarchy to the original purposes depends on this isomorphism.

Similarly the idea may suggest itself to admit the distinction of sense and denotation at the nth level and above in the hierarchy of types, but below the nth level to deny this distinction and to adopt instead Russell's device of contextual elimination of names. The entities assumed would thus include only the usual extensional entities below the nth level, but at the nth level and above they would include also concepts, concepts of concepts, and so on. However, this is no reduction in the variety of entities assumed, as compared to the theory which assumes at all levels in the hierarchy of types not only the extensional entities but also concepts of them, concepts of concepts of them, and so on. For the entities assumed by the former theory are reduced again to isomorphism with those assumed by the latter, if all entities

below the nth level are deleted and appropriate deletions are made in every type at the nth level and above.

Some one may object that the notion of isomorphism is irrelevant which is here introduced, and insist that any subtraction from the entities assumed by a theory must be considered a simplification. But to such objector I would reply that his proposal leads (in the cases just named, and others) to perpetual oscillation between two theories T_1 and T_2, T_1 being reduced to T_2 and T_2 to T_1 by successive "simplifications" *ad infinitum*.

[16] As is done in the forthcoming revised edition of my *Introduction to Mathematical Logic, Part I.*

[17] For convenience, English is used also as the meta-language, although this gives a false appearance of triviality or obviousness to some of the semantical rules. Since the purpose is only illustrative, the danger of semantical antinomies is ignored.

[18] For present illustrative purposes the question may be avoided whether common nouns in English, in the singular, shall be considered to be variables (e.g., 'planet' or 'a planet' as a variable having planets as its range), or to be class names (e.g., 'planet' as a proper name of the class of planets), or to have "no status at all in a logical grammar" (see Quine's *Methods of Logic*, p. 207), or perhaps to vary from one of these uses to another according to context.

[19] Or possibly 'planet' and 's' could be regarded as two primitive symbols, by making a minor change in existing English so that all common nouns form the plural by adding 's.'

[20] If any of you finds unacceptable the conclusion that therefore 'the number of planets is round' is a sentence, he may try to alter the rules to suit, perhaps by distinguishing different types of terms. This is an example of a doubtful point, on the decision of which there may well be differences of opinion. The advocate of a set-theoretic language may decide one way and the advocate of type theory another, but it is hard to say that either decision is the "true" decision for the English language as it is.

[21] But of course it would be wrong to include as a rule of denotation: 'the world is round' denotes truth. For this depends on a fact of geography extraneous to semantics (namely that the world is round).